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Smith. Nolen '84.

ESSAYS

ON

VIVISECTION.

Anat.

VIVISECTION,

SCIENTIFICALLY AND ETHICALLY CONSIDERED

IN

Prize Essays,

BY

JAMES MACAULAY, A.M., M.D.,
FELLOW OF THE ROYAL COLLEGE OF SURGEONS OF EDINBURGH

REV. BREWIN GRANT, B.A.,
VICAR, ST. PAUL'S, BETHNAL GREEN, LONDON, E.;

AND

ABIATHAR WALL,
LICENTIATE OF THE ROYAL COLLEGE OF PHYSICIANS, EDINBURGH
MEMBER OF THE ROYAL COLLEGE, ENGLAND, &c.

LONDON:
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"The Queen hears and reads with horror of the sufferings which the brute creation often undergo from the thoughtlessness of the ignorant, and she fears also sometimes from experiments in the pursuit of science."— *Extract from Letter, addressed by command of Her Most Gracious Majesty the Queen to the President of the Royal Society for the Prevention of Cruelty to Animals, by* Lieutenant-General Sir T. M. BIDDULPH, K.C.B., *dated Buckingham Palace, 10th June, 1874.*

"What took place in this country was comparatively little when compared with what took place on the Continent; and it was the enormities of Continental practices which led us to inquire what was taking place around us, and the results were sufficiently horrible."—LORD CARNARVON, *on moving the second reading of the Anti-Vivisection Bill in the House of Lords, May 22nd, 1876. —British Medical Journal, May 27th, 1876.*

"You have said that the outcry was a sentimental one ("Yes") —well, that sentimentality will hold ever."—Right Hon. R. A. CROSS, *Home Secretary, in reply to the Medical Deputation, 10th July, 1876.—British Medical Journal, 15th July, 1876.*

These Essays

ARE RESPECTFULLY DEDICATED BY THE DONOR,

TO THE JUDGES,

AND TO

THOSE MEDICAL MEN WHOSE SENSE OF RIGHT AND MORAL COURAGE HAVE ENABLED THEM TO BREAK THE FETTERS OF PROFESSIONAL CASTE, AND, ON THIS GRAVE SUBJECT, TO DECLARE THEMSELVES ALIKE FOR THE CREATOR AND THE CREATURE.

NAMES OF THE JUDGES.

W. A. F. BROWNE, Esq., LL.D., F.R.C.S.E., formerly Medical Commissioner in Lunacy for Scotland, Crindau, Dumfries.

ARTHUR DE NOÉ WALKER, Esq., M.D., M.R.C.S. Eng.; L.A.H. Member of the Royal Institution of Great Britain, 10 Ovington Gardens, London.

JAMES COWIE, Esq., M.R.C.V.S., late Vice-President, Member of Council and of the Board of Examiners of the Royal Veterinary College of Surgeons, London—Sundridge Hall, Bromley, Kent.

JOSEPH JOHNSTON, Esq., M.D., Surgeon-Major, Army Medical Department, 3 Lorne Terrace, Dublin.

JAMES GILCHRIST, Esq., M.D., Member Botanical Society, Edinburgh; Medical Superintendent of Crichton Royal Institution, Dumfries.

DAVID JOHNSTON, Esq., M.A., M.D., L.R.C.S. Edinburgh—Kair House, Fordoun, Kincardineshire.

JOHN H. BRIDGES, Esq., M.B., F.R.C.P. London; late Fellow of Oriel College, Oxford; Medical Inspector Local Government Board, Whitehall, London.

PREFACE.

THE following Essays were written in response to an offer of Two Hundred Guineas for the best Essay on "Painful Experiments on Living Animals, Scientifically and Ethically considered."

The Theses of the Essay were given in the announcement as follows:—

1. Have such experiments been of any scientific value? Are there not fallacies underlying such a method of interrogating Nature which, of necessity, vitiate the results?

2. Assuming, for the sake of argument, that such experiments may have been of scientific value, or may have led to the discovery of scientific facts of permanent importance, could such discoveries not have been arrived at by a broad and comprehensive study of natural phenomena, or of those *quasi*-natural facts which are the continual accompaniments of civilisation? If so, would the latter method of inquiry necessarily have much retarded the dates of such discoveries?

3. Are such experiments morally justifiable? Is not their tendency to harden the operator, and blunt his moral sense? Are they compatible with the moral government of the universe, so far as that can be inferred from the facts of Nature?

The Essays given in were adjudicated upon by seven eminent gentlemen whose names will be found on another

page, and who kindly consented to act as Judges. They examined the Essays submitted to them with scrupulous care, and the conclusion arrived at was, that each of the three Essays published in this volume had two Judges in its favour as the best. In these circumstances, the seventh Judge did not feel warranted to decide the question, and thought it better, with the consent of all parties, to divide the award among the three authors, and publish their Essays. This has secured a fuller treatment of the subject than could have been done by the work of one author, which is a gain to the cause. It will thus be seen that the order in which the Essays appear in this volume does not indicate the order of merit; for they had each the suffrages of the same number of Judges.

It may also be stated that Dr. Gimson's Essay on the same subject, though written according to the Theses of these Essays, and originally intended for the prize, was withdrawn before adjudicated upon, and published by himself.

EDINBURGH, *January*, 1881.

TITLES OF ESSAYS.

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VIVISECTION:

IS IT SCIENTIFICALLY USEFUL OR MORALLY JUSTIFIABLE?

AN ESSAY

ADDRESSED SPECIALLY TO THE MEDICAL PROFESSION.

BY

JAMES MACAULAY, A.M., M.D.,

Fellow of the Royal College of Surgeons of Edinburgh.

Ex iis quæ violentiâ quæruntur, alia non possunt omnino cognosci, alia possunt etiam sine scelere.—*Celsus.*

TITLE OF THE ESSAY:—

"PAINFUL EXPERIMENTS ON LIVING ANIMALS SCIENTIFICALLY & ETHICALLY CONSIDERED."

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1. Have such experiments been of any scientific value? Are there not fallacies underlying such a method of interrogating Nature which, of necessity, vitiate the results?

2. Assuming, for the sake of argument, that such experiments may have been of scientific value, or may have led to the discovery of scientific facts of permanent importance, could such discoveries not have been arrived at by a broad and comprehensive study of natural phenomena, or of those *quasi*-natural facts which are the continual accompaniments of civilisation? If so, would the latter method of inquiry necessarily have much retarded the dates of such discoveries?

3. Are such experiments morally justifiable? Is not their tendency to harden the operator, and blunt his moral sense? Are they compatible with the moral government of the universe, so far as that can be inferred from the facts of Nature?

PLAN OF THE ESSAY.

Medical opinion in this country, formerly strongly opposed to experiments on living animals, appears now to be mostly favourable to this mode of research. Professor Rutherford's address to the British Medical Association.

Explanation given of this change in professional opinion. Remarks on the use of anæsthetics.

Importance of the subject being *now* considered by the medical profession, before the continental usages are established in our medical schools.

Sir James Y. Simpson's celebrated Address "On the Modern Advancement of Physic." In describing the causes of past progress, and the hopes for the future, Sir James Simpson makes not one word of reference to experiments on living animals; all is ascribed to the usual methods of medical and scientific research.

Examination of some of the stock arguments in favour of experiments on living animals. The Circulation of the Blood—Double function of the Spinal Nerves—Use of Anæsthetics—Vaccination. All of these due to observation as distinguished from experiment.

Recent legislation as to Vivisection, and inspection of Physiological laboratories, the result of popular agitation, not of professional opinion. The Norwich prosecution. Experimenters put upon their defence.

Article in *British Medical Journal* headed, "What has Vivisection done for Humanity?" This article laid in evidence before

the Royal Commissioners, and by them published in the Blue Book. Analysis and examination of this list of discoveries alleged to be due to experiments on living animals, and of other improvements in medicine and surgery ascribed to this mode of research.

Assuming that some facts of scientific value have been established by experiment, could they not have been ascertained in other more legitimate ways? Proofs and illustrations of the superiority of the method by observation in the human body, in health and disease, in life and after death, over the method by experiment on lower animals, under unnatural conditions. Examples and testimonies.

Fallacies inseparable from this method of research. Examples of uncertain and of contradictory results, and of results that have been misleading and mischievous.

Are such experiments morally justifiable? Statement of the ethics of the case. The nature and extent of man's dominion over the lower animals considered. Man's rights of dominion not absolute, but limited and conditional. The conditions of the use of animals for experiments examined. The relations of this mode of research to moral law as well as intellectual progress to be considered. Not in harmony with the moral government of the world, and the eternal principles of justice and mercy.

Reflex effects considered. The tendency of Vivisection to harden the operator and to blunt the moral sense. This proved to be the result of the system, whatever it may be in regard to some individuals. Injurious also to the medical profession generally, and dangerous to society. Proofs and testimonies, even from those not wholly opposing experiments. Appeal to medical men to judge the matter on ground of science, and the verdict will agree with public sentiment, relieving the profession from the opprobrium that has come upon it through the practice of Vivisection.

VIVISECTION.

A GREAT change of opinion appears to have taken place among the medical profession in England on the subject of Vivisection. In the *Medico-Chirurgical Review* for 1842, Mr. Shaw, now (1880) the veteran Consulting Surgeon to the Middlesex Hospital, in giving a summary of his kinsman's, Sir Charles Bell's, researches, thus expressed himself:—"The profession must be well persuaded by this time what a difficult task it is to obtain any uniform results by having recourse to experiments on living animals. And it is scarcely too much to say that, if physiologists had waited patiently till cases occurred in practice, such as have actually been met with in very numerous instances, when the pathological phenomena confirmed the views deduced from anatomy, our convictions would be as strong as after all the multiplied experiments which have been performed."

The words of Sir Charles Bell himself are still more emphatic:—"In a foreign review of my former papers," he says, "the results have been considered as in favour of experimenting on living animals. They are, on the contrary, deductions from anatomy; and I have had recourse to experiments, not to form my opinions, but to impress them upon others. It must be my apology that my utmost powers of persuasion were lost while I urged my statements on the ground of anatomy alone." And again, "Experiments have never been the means of discovery, and a survey of what has been attempted of late years will prove that the

opening of living animals has done more to perpetuate error than to enforce the just views taken from anatomy and the natural sciences."—(Bell on the Nervous System.)

I have before me a printed circular, signed by thirty-eight medical men resident at Bath, which shows what was the general feeling in the profession on this subject, at a somewhat earlier period. It is dated Bath, February 27, 1825:—"We, whose names are under-written, medical persons, chiefly practitioners, resident at Bath, do hereby engage and declare that we will, as far as in us lies, prevent and discourage by our example, influence, and dissuasion, those painful and cruel anatomical experiments upon living animals, which to the disgrace of science, in our opinion, are made, sometimes without necessity or utility, and frequently without any adequate end, under the plea of promoting medical knowledge. . . . We do thus protest against and reprobate such conduct, esteeming it wholly unwarrantable and discreditable to our profession."

In the journal already quoted—the *Medico-Chirurgical Review* (vol. xxxvi., new series)—at that time the leading organ of the profession, the editors, after giving some account of M. Longet's experiments, says:—"We cannot conceal our abhorrent dislike of what the French call Vivisection, in which unoffending brutes are made the victims of the most shocking sufferings, all with the view of advancing science!" More is said, in a tone of earnest indignation, with which the majority of readers in those days, no doubt, heartily sympathised.

Even so recently as 1866, when prize essays on Vivisection were published by the "Royal Society for the Prevention of Cruelty to Animals," the author of one of the essays, Dr. Markham, Physician to St. Mary's Hospital, London, while not denying the abstract right to make experiments, nor their occasional utility, observes:—"I need hardly say that courses of experimental physiology are nowhere given in this country; and that my remarks, con-

sequently, apply only to those schools in France, and elsewhere, where such demonstrations are delivered."

In the few years that have since passed, the practice of vivisection has not only greatly increased in this country, but seems now to be regarded with altered feelings by a large part of the medical profession. There were always some physiologists and surgeons who thought it right to use this method of investigation; but their researches were quietly conducted, and not with ostentatious publicity, as in Paris and other Continental cities. The whole tone of English professional opinion was against such experiments. English medical students would have revolted against such exhibitions as were customary in foreign schools. But a change has been gradually coming over the spirit of the profession. Courses of "demonstrations in animal physiology" are given in various medical schools. "Handbooks" are published for the use of pupils in "physiological laboratories." The extent to which this mode of investigation, so recently introduced, is carried on will never be fully known; but it is already so far a department of study and education that physiological laboratories, like anatomical class-rooms, are under legal regulation and official inspection.

The tone of the medical press, which may be supposed to represent professional opinion, is also very different from what it used to be. To give but one instance—an article in the *British and Foreign Medico-Chirurgical Review*, for April, 1875, a journal holding a position analogous to that of the old *Medico-Chirurgical*, not only advocates the free practice of vivisection, but deals with objections to it as arising only from ignorance or fanaticism. The tone of the press is communicated to the profession. It may be that, at Bath, and at other centres of the Provincial Medical Association, there may be forty men ready to sign a protest as clear and firm as that which we have quoted. But the

published proceedings of such bodies do not favour this hope. At the annual meeting of the British Medical Association, held at Edinburgh, in 1875, under the presidency of Sir Robert Christison, an address was delivered by Professor Rutherford, of the University of Edinburgh, who has attained eminence as an experimenter on living animals. "In recent years," said Professor Rutherford, "the teaching of physiology has made a great stride in this country. Laboratories, duly appointed, have been and are being organised. The method of physiological instruction has, in most instances, risen from the mere prelection illustrated by diagram, to the experimental illustration of the subject. I cannot suppose that any member of this Association entertains the idea that experiments on the lower animals are not justifiable for the discovery of new truth; but I am aware that there are some who entertain the idea that vivisection is not necessary when it has for its object the mere demonstration of educational principles and facts already known. Those who hold this doctrine appear to me to forget that *physiology is an experimental science*, and that no right conception of the subject can be obtained unless the students be shown the experiments that are necessary for the demonstration of certain facts."

Professor Rutherford concluded his address by describing numerous experiments which he had made as to the effects of various medicines on the secretion of bile in the healthy dog. His description of the experiments was illustrated by diagrams, which appeared to convey to the meeting a sufficiently clear notion of his researches, and certainly in a way less disagreeable than witnessing the experiments themselves. It was a practical refutation of his own assertion that seeing the operations was essential to a right understanding of them. To these experiments I shall afterwards have to refer, but meanwhile have quoted a brief portion of the address for the sake of making a few comments.

In the first place, the Professor asserts that "physiology is an experimental science," and that, therefore, experiments on living animals must be made and must be exhibited. The fallacy in this statement is, that *experimental* is an epithet here used in a wrong sense. "Experimental science" is a synonym for "*inductive* science," or science based on the observation of facts. It is a *petitio principii* to assume that vivisection is necessary to constitute physiology one of the experimental or inductive sciences.

But passing from this, can it be said with truth that the Institutes of Medicine, or Physiology, cannot be intelligently taught without the exhibition of experiments on living animals? Professor W. P. Alison, the distinguished predecessor of Dr. Rutherford, never made such demonstrations, and he was a man as distinguished as a physician as he was successful as a teacher.

Another eminent teacher in the Edinburgh Medical School, Dr. John Fletcher, in the introductory lecture to his course on physiology and on medical jurisprudence, gave a testimony in direct opposition to that of Dr. Rutherford. "None of the functions of animals need be seen in action, in order to be perfectly well understood: they may be abundantly well fancied from preparations and representations of the organs engaged in performing them—and none, certainly, will be exhibited in action in the present lectures.

"During many years experience in lecturing on this subject, and in delivering courses of more than ten or twelve times the duration proposed at present, I have never yet found it necessary, in a single instance, to expose a suffering animal, even to students of medicine (who are necessarily, in some degree, familiarised with sights of horror), for the purpose of elucidating any point of physiology, and I certainly shall not begin now; nor can I refrain from stating my belief, that experiments on living animals are much less necessary, even to the advancement of this science, than has been sometimes imagined. I am perfectly aware how much

this plan of "interrogating Nature" has done, in modern times, for every branch of physical science; but I am equally persuaded that these advantages have been, in general, overrated — at any rate that students, in this respect, generally begin at the wrong end, and are often engaged in experimenting on animals, in hope of finding out something or other on which to found some new and surprising doctrine, while they take no manner of notice of the great number of things continually going on in their own bodies, of the rationale of which they are ignorant.

"It was a precept which I learned from my first teacher in medicine, the late venerable Abernethy, constantly to remember that I carried always about with me the best subject for observation and experiment — one the most easily to be consulted, since it was quite in my power, and one the phenomena of which should be the most interesting to me, since it was with similar beings alone that I should in future have any immediate concern; and this precept I have never lost sight of. We ought never to forget that the best subject for analysis is ourselves, and the most useful contemplation that which relates to the most common processes; and that, till we understand all which can be readily understood, with a little reflection, about ourselves, and know the rationalia of all familiar phenomena, it is preposterous to pore over the warm and quivering limbs of other animals, in search of things recondite and comparatively useless." (Introductory Lecture.)

This testimony of an experienced and successful teacher of physiology disposes of the alleged necessity for demonstrations on living animals, for purposes of explaining the facts and principles of the science. On the same point Professor Owen has recorded his opinion in these emphatic words:—"I reprobate the performance of experiments on living animals to show to students what such experiments have taught the master; whilst the arguments for learning to experiment, by repeating experiments on living animals,

are as futile as those for so learning to operate chirurgically." Professor Owen thus expressed his opinion in explaining his award in the competition for the prize essay in 1866.

I have also an equally clear statement of opinion in a letter from my old teacher, Sir Robert Christison. His words are these:—"I object to all public demonstrations by experiment on living animals, and have always done so." Sir Robert did not utter a similar protest before the British Medical Association, though he might have taken the opportunity. But there was a good deal of irritation at the time, in prospect of legislative interference; and the profession was so far put in a defensive attitude, on account of the agitation against vivisection. The consequence was, that Professor Rutherford's address was received with apparently the unanimous approval of those present, and the report of the meeting contains no reference to any protest having been made.

It becomes an important and fitting matter for inquiry how this undoubted change in the opinion of the profession on the subject of vivisection has been brought about. How is it that so many are now advocating this new method of research, instead of keeping in the old and safe paths of observation?

The change is certainly not due to any notable discoveries made in recent years by vivisection, nor any improvements in medical practice resulting therefrom. Still, it is interesting to inquire why the strong feeling against experimenting on living animals, which once honourably marked the profession in England, has been weakened; and why our schools of physiology are assuming greater resemblance to the once much-censured schools of the Continent.

In attempting an explanation, I think that several things must be taken into account. In the first place, there is the natural and laudable desire for the advancement both of medical knowledge and practice. The path of progress by clinical and by pathological research, though safe and sure,

involves careful and patient research, as all inductive science requires. The numerous and marvellous strides made in other departments of practical science during the last half century throw into marked contrast the comparatively slow progress of medicine. To use the words of Sir James Y Simpson—"Ever and anon we hear it doubted, by men both without and within the profession, whether medicine has made any marked progress at all during the period that I speak of. Most of us have heard it broadly insinuated that while other departments of science and art have, during the last fifty or sixty years, been marching forwards at a pace unprecedented in their history, the art of healing has remained comparatively stationary."

To Sir James Simpson's Address "On the Modern Advancement of Physic," delivered from the Presidential chair of the Edinburgh Medico-Chirurgical Society, I shall refer presently. These sentences quoted from it express well the too prevalent feeling as to the comparatively slow progress of medicine. It is natural that both physiologists and practitioners, chafing under this feeling, hail any prospect of accelerated progress, and lend a ready ear to the assertions of those who proclaim that by vivisection they have found a shorter road to knowledge.

Another influence is to be taken into account in explaining the present attitude of the profession. The example of foreign schools in their curricula of study have been followed of late years more than it used to be in this country. "Demonstrations" in animal physiology have been introduced in various medical schools. "Physological laboratories," and other institutions for experimental research, have been established and endowed. English as well as foreign pupils of Continental laboratories are engaged in giving practical instruction to students. Some of these professors and demonstrators are Fellows of our own Colleges of Physicians and Surgeons, and members of the Senates of our Universities. These are men of very different habits and

character from some of the Continental experimenters, whose proceedings formerly caused honest indignation, but whose methods of research they are introducing. Through association and fellowship with them, there has arisen a strong *esprit de corps* in the profession, leading many to defend their brethren from attacks which have been sometimes unfair, and from charges which have been sometimes unjust. Knowing that the practice of vivisection was followed in an honest and sincere desire for the advancement of science, sympathy has been felt for the experimental physiologists, even by many who disapproved of their method of research. Expression of this sympathy by Medical Councils and Societies has given the appearance of a general feeling widely at variance with the "indignation and abhorrent dislike" expressed by the Editors of the *Medico-Chirurgical Review*, in the passage already quoted, and which undoubtedly represented the opinions of the profession at that period.

A third and more marked element in the change of opinion is the discovery of anæsthetics, and their use in the performance of experiments. It is generally supposed that the use of chloroform renders impossible the horrible cruelties, especially in French laboratories, the reports of which caused Englishmen to view the whole system of vivisection with pain and dislike. The introduction of anæsthetics has thus lessened the antipathy and quieted the opposition of many professional men. But it has at the same time diverted attention from the main question, from a scientific point of view, whether vivisection is a legitimate method of research, under whatever conditions. The object of this essay is to maintain the negative, and this both on scientific and ethical grounds.

It may be well here to clear the way by a few further remarks on the use of anæsthetics in vivisection. The phrase, "painful experiments," may lead to misunderstanding. In some experiments anæsthetics are used, in others they

are not used, and, in fact, would interfere with the results. In places registered under the Vivisection Act, the use of them is left to the conscience and judgment of the licensed operator. The majority of experiments, as of the public demonstrations, may be called painless; but vast numbers are not so. Many of them extend over long periods of time, during which the effect of chloroform has passed off. It is not always that the animals are "mercifully put out of pain," as one physiologist tells us is the usage at Guy's Hospital. In other London hospitals, experiments are on record where the investigations lasted day and night for weeks together. The readers of medical journals know that animals are often kept alive in a mutilated state for the repetition or variation of experiments. Take but one instance from the *Handbook of the Physiological Laboratory* (p. 403), a demonstration upon "Recurrent sensibility:" "This can be shown only in the higher animals, the cat or dog being best adapted for the purpose. The method adopted is this: The arches of one or two vertebræ are carefully sawn through, or cut through with the bone forceps, and the exposed roots very carefully freed from the connective tissue surrounding them. *If the animals be strong, and have thoroughly recovered from the chloroform, and from the operation*, irritation of the peripheral stump of the anterior root causes not only contraction in the muscles, but also movements in other parts of the body, indicative of pain. On dividing the mixed trunk the contractions cease, but the general signs of pain or sensation remain."

The Blue Book of the Royal Commission on Vivisection contains many similar facts. Dr. Klein stated, in reply to question as to use of chloroform (3605), "I prefer and use chloral hydrate; but as a general rule, for my scientific investigations, I do not use chloroform, or any other anæsthetic, except for convenience sake, in dogs and cats, and for no other animals as a general rule." Being asked

(3631) if he did not perform operations which involved a great deal of pain to the animal, the answer was: "Not as operations, but in their eventual results, we do occasionally."

Ah! it is these "eventual results" that are mainly to be considered in this question of anæsthetics. The knife may be used while the animal is under the influence of chloroform; but what of the resultant injury and mutilation, and the consequences of the experiments? Life, if not destroyed "mercifully," may be made miserable for the poor creatures. The words of Dr. George Hoggan sound strangely paradoxical at first, but they convey a sad truth nevertheless:—"*I am inclined to look upon anæsthetics as the greatest curse to vivisectable animals.* They alter too much," he explains, "the normal conditions of life to give accurate results, and they are therefore little depended upon. They, indeed, prove far more efficacious in lulling public feeling towards the vivisectors than pain in the vivisected."

So much for "painless experiments." With few and unimportant exceptions, I hold that all experiments on living animals—all, at least, to which objection is made in this essay—are painful; painful either at the time or in "eventual results," whether these be mutilation, disease, or death. I hope to show that they are scientifically needless and ethically wrong, and that therefore vivisection ought to be discouraged and condemned by the medical profession.

It may be thought by some that this is an unseasonable time to renew or to extend the agitation against vivisection. An Act of Parliament, they say, has been passed, as the result of a Royal Commission of inquiry, and is now in operation, after being accepted, if not approved, by the representative bodies of the medical profession. The proceedings of experimenters are under regulations, imposed by the collective wisdom of Parliament, and with Inspectors to exercise wholesome supervision and control. Places where experiments are performed must be registered; experimen-

ters must have licenses, either ordinary or special; and reports are made by the inspectors. Why not wait to see how the Act works? Such is at present the *laissez faire* tone of professional opinion, and it is largely shared by the general public.

In opposition to this spirit of indifference, I maintain that the sooner and the more fully this matter is discussed the better. And this not in the cause of humanity only, but in the interests of science, and for the honour of our profession.

If this new system of research and of instruction is wrong, let it not have time to take deep root and to spread in our medical schools. To the credit of the profession in Ireland, the programme of the practical course of Institutes of Medicine, under the joint-direction of Trinity College, Dublin, and the College of Physicians, concludes with the significant "*N.B.—'Vivisections are absolutely prohibited.'*" Even if this prohibition is still maintained in Ireland, we fear that it will not for some time be imitated in other schools. The number of licensed vivisectors may vary from year to year, but the names of those to whom licenses are granted are kept secret, and the reports of the Inspectors are not open to the public. No one can tell the nature or extent of the experimental researches, except so far as the operators choose to record them in medical journals, as Professor Rutherford has done; or to bring them before scientific societies, as Professor Ferrier has done in his Reports to the British Association. The publication of such experiments is is sure to give a fresh impulse to research by vivisection.

I think, therefore, the time has come for making an appeal to the medical profession for a calm inquiry as to the position and claims of the system. Enough has been done to bring the matter before the general public. There is no fear of the agitation out of doors being at an end, although the advocates of vivisection seem to think that the licensing of laboratories has silenced their opponents. No Act of Parliament can suppress public sentiment on this

question. It will not be wise in the medical profession to set itself in direct and obstinate opposition to this public sentiment. If, on the one side, there has been too much unintelligent clamour against vivisection, there has been, on the other side, too little of fair discussion of the merits of the case. At the same time, a very large number of medical men have not committed themselves to the advocacy of vivisection. The great majority, I am certain, have not specially considered the subject, and have not any feeling beyond unwillingness to separate themselves from their brethren, when attacks seem to be made on the profession.

I put the question lately to the senior physician of one of our great London hospitals, if he thought vivisection had added anything to our resources which might not have been otherwise obtained, and his reply was that he had not studied the matter so as to give an answer. Another physician, occupying one of the highest positions in the profession, on my asking him about some alleged physiological discoveries, said he must enquire from his friend, P. S——, naming a surgeon and experimenter of Guy's Hospital. In the same way I have tested other medical friends, and find they are at a loss to name any practical benefits derived from vivisection. They are told that important investigations are instituted, and they are unwilling to object to any mode of research which is said to give promise of results. Comparatively few have personally studied the question, or have ventured openly to express doubt or disapproval. I believe there must be many who would be willing to see the system fairly examined, and who would even be glad to find that the result of this examination was in harmony with public sentiment, and with the former all but unanimous opinion of the medical profession in England.

It is to these men, not committed to the advocacy of vivisection, but willing to hear what can be said against as well as for it, that I address myself. It would be far

more easy to write a large volume than a short essay with this purpose. In describing and analysing the reports of physiological laboratories it would be easy to multiply proofs and illustrations of the fallacies underlying this mode of inquiry, and to point out the contradictory results of different experimenters. It would be easy, also, to gather from the records of scientific research and medical practice a great mass of observed facts and phenomena, establishing all the important conclusions which vivisection claims as discoveries. But to enter into voluminous details or minute arguments would defeat the purpose of this essay. The design of the writer is to state briefly but clearly the principles of the controversy; and by showing that vivisection is indefensible, on the ground of science as well as of sentiment, to urge medical men to re-occupy the same position which was honourably maintained by the leaders of the profession in England before this new invasion from foreign schools of physiology.

Reference has already been made to the Address by Sir James Y. Simpson on "The Modern Advancement of Physic." It is a bright and cheering record of progress in the healing art during the first half of the present century. If any one doubts whether medicine has made marked advance, or is ever despondent as to its prospects in the future, let him read that essay, and he will find proofs of progress as great and rapid as in any department of knowledge or art. It is a retrospect at once instructive and encouraging. The enumeration of improvements both in medicine and surgery will surprise those who have not considered the state of science and of practice at an earlier period than that passed under review. Without going into many details, a few of the results may be noted.

After the middle of last century the mortality of children under five years of age, in London, was above fifty in the hundred. It is now not more than from thirty to thirty-five.

The saving of life by improvement in the hygeine and management of infancy is now more than 100,000 human beings a year throughout Great Britain. The average mortality at all ages, and especially in towns, has remarkably decreased; and the chances of life have steadily increased. Some of the diseases which were formerly among the most fatal in the bills of mortality, scurvy, dysentery, ague, and small-pox, are now low in the lists. The treatment of actual disease is only one department of practical medicine. The preservation of health and the prolongation of life are equally important. These objects are attained on the large scale by the prevention of disease much more than by its cure. It may be long before specific cures are found for other fatal diseases, as effective as those which have checked the mortality from ague, scurvy, and small-pox. "But does not the history of the past," says Sir James Y. Simpson, "encourage us to a bold belief that our present most fatal diseases may, by the advancement of hygeinic and medical means, be our most fatal diseases no longer?" . . I confess that I cannot but entertain an ardent belief that medical science may yet devise measures, prophylactic perhaps rather than curative, to stay the great destruction of human life prevailing amongst us from the most fatal of these affections—phthisis. Perhaps a more advanced pathology and chemistry may yet ere long furnish us with more enlightened views of pneumonia and other inflammatory disorders than we yet possess, and arm us with more sure and potent medicinal weapons and resources against them. We have, from the experience of the last few years, every reason to hope that the whole class of zymotic diseases will be greatly subdued betimes in intensity and violence when the investigation of the physical causes predisposing to them, or even actually exciting them, is more fully expiscated.

"Besides, if by vaccination during infancy, medicine has devised means to arrest the ravages of small-pox, may it not

yet devise means also, by inoculation or otherwise, to arrest the ravages of scarlet fever and measles, of hooping-cough, of typhus fever, and perhaps of the whole class of non-recurrent diseases? And even if we fail to arrest them, we may possibly find out for the various animal poisons producing these diseases, antidotes as certain as quinine and arsenic are antidotes against the poison of marsh fever.

"Let us at least not sit indolently down and argue ourselves into the belief that it is impossible to attain such results. The conquest of small-pox seemed to our forefathers, a hundred years ago, as impossible as the conquest of these maladies can look to any one now; and yet we all know that the subjugation of small-pox was effected by the genius of one man, and by the devotion of one mind to its accomplishment.

"Some time before Jenner turned his attention to the subject, the learned and accomplished Dr. Mead, the first London physician of his day, wrote of the utter hopelessness of the very idea of battling with and vanquishing such a formidable enemy to human life and happiness as small-pox. He speaks of the possibility of 'a specific antidote being found against the contagions of small-pox;' that is, an antidote 'by which it may be so thoroughly destroyed that, though it had been received into the body, it may not produce the disease,' as an idea as wild and chimerical as that of alchemy; and one, in his opinion, 'outraging the principles and elements of things that are so certain and well-established by the permanent laws of nature.'

"These disheartening opinions of Dr. Mead, regarding the hopelessness of ever gaining a prophylactic for small-pox, were published in 1747. Before fifty years had elapsed Jenner had both discovered and successfully applied to practice the great prophylactic measure that has rendered his name imperishable in the annals of the human race.

"Meanwhile, the prevention of diseases by the methodised avoidance of their causes has made a mighty advance during

the last twenty or thirty years. Where the preceding causes of disease have been set aside from special communities by proper sanitary arrangements, human life has, in such communities, been prolonged, and the physical as well as moral health and happiness of the inhabitants been correspondingly ameliorated."

The progress of surgery has been not less marked than that of medicine; and Sir James Y. Simpson gives a brilliant enumeration of improvements both as to operations and general treatment. In operative surgery, the abrogation of pain and suffering by anæsthetics has been a wonderful improvement; but an even greater mark of progress is the increasing endeavour to heal and to cure, without operations, cases and diseases in which operations were formerly considered indispensable. More than ever is surgery associated with medicine in the object of preservation and cure. And where operations are still required, the surgeon knows that, in eight or nine cases out of ten, the risk is not from surgical lesions but from constitutional complications of a truly medical nature. Hence, both surgery and medicine are indebted for their progress to the better knowledge of principles which underlie every department of the healing art. "At the present day," says Sir James, "we can scarcely appreciate the vast importance of some of these branches of study, and the advantage which a knowledge of them gives us as practitioners over the cultivators of medicine half a century ago. Nor, perhaps, would it be possible to see and appreciate them in their proper value, unless we were actually again deprived of their aid—in pathology, diagnosis, and practice—and unless all the knowledge and advantages springing from them came to be suddenly obliterated and blotted out."

What, then, are the departments of research which Sir James Y. Simpson specifies as having led to the modern

advancement of physic, and which give hope of future progress? The first is pathological or morbid anatomy, a branch of study, in its systematised form, almost wholly of modern growth. Secondly, pathological histology has opened up a wide field of knowledge concerning the origin, character, and courses of different diseases, and diseased actions and structures. A third department of research is that of pathological chemistry, which has afforded new and important information regarding the actual character and nature of disease. Along with these three departments of medical science — pathological anatomy, pathological histology, and pathological chemistry — the practitioner has acquired new means of physical diagnosis, by which to detect the presence or effects of morbid conditions in the living patient. The use of the microscope, and of various chemical tests to the fluid excretions of the body, has helped to improve the diagnosis of disease. Nor must we omit the improvements in materia medica, whether in the form of the remedies or in the methods of applying them, so as to exert their medicinal influence upon the body, or upon the different organs or functions of it.

On all these, and on other points, the essay of Sir James Simpson gives gratifying testimony of recent progress, with encouraging anticipations of the future. No physician in our times has been more fully acquainted with all the discoveries and researches both of English and foreign workers and authors. Yet the address contains not one word about experiments upon living animals, not one reference to those "physiological laboratories" to which many are now looking for new knowledge and power. The eye is directed throughout to the researches of legitimate science, and no help expected from the lurid light of vivisection.

There are many, however, who have a vague idea that the beginning of all this progress was due to experiments on living animals. Let us examine the instance which is always

put in the front by advocates of vivisection—the discovery of the circulation of the blood. It appears in every list of alleged discoveries from this mode of research, and would probably be the first mentioned in any controversy on the subject. When Sir William Gull was asked by the Royal Commissioners if he could "enumerate any considerable number of therapeutic remedies which have been discovered by this process of vivisection?" the answer was—"The cases bristle around us everywhere; our knowledge of dropsical affections, of pulmonary apoplexy, of enlargement of the liver, and the whole category of such affections, was due to Harvey's discovery of the circulation." Here vivisection gets credited with not only Harvey's discovery, but with all the consequences of the knowledge of the circulation of the blood! But what if this discovery was not wholly due to vivisection?

It is not necessary, in examining this question, to depreciate the claim of Harvey to great renown, nor to inquire how far the discovery was anticipated by others, or what share they have in the discovery. The only point here to be discussed is, "Was the discovery due to vivisection?" The Royal Commissioners say that "Harvey *appears to have been almost entirely* indebted to vivisection for the ever-memorable discovery of the circulation of the blood"—the old and constant re-iteration, but with more cautious assertion than is usual.

Harvey himself did not rest his entire claims on vivisection. "I remember," writes Robert Boyle, "that when I asked our famous Harvey, in the only discourse I had with him (which was but a little while before he died), what were the things that induced him to think of a circulation of the blood, he assured me that when he took notice that the valves in the veins of so many parts of the body were so placed that they gave free passage to the blood towards the heart, but opposed the passage of the blood the contrary way, he was invited to think that so provident a cause as

Nature had not placed so many valves without a design; and no design seemed more probable than that, since the blood could not, because of the interposing valves, be sent by the veins to the limbs, it should be sent through the arteries, and return through the veins whose valves did not oppose the course that way." It is probable that some vivisectors do not know who Robert Boyle is, or why his testimony is of weight, but those who do will not undervalue this record of Harvey's own account of what led to the discovery. It was Fabricius, of Padua, Harvey's master in anatomy, who pointed out to him this arrangement of the valves, but Harvey's genius led him to connect it with the various facts of the circulation already known to Cesalpino, Servetus, and other observers. To his students at Pisa and at Rome, Cesalpino taught the circulation from the veins to the right side of the heart, thence to the lungs, and from the lungs to the left side of the heart, and to the arteries. The astonishing thing is that the complete discovery was so long delayed, not that it came when it did. The state of science in England, far behind that of Italy before the middle of the seventeenth century, caused Harvey's announcements to be received with wondering admiration. But he neither began nor completed the discovery by his experiments on living animals. He exhibited some points already known to Italian physicians, but his demonstrations failed to convince such men as Riolan, of Paris, and Hoffman, of Nuremberg. Even Dr. Willis, the biographer of Harvey, admits that "he left the doctrine of the circulation as an inference or induction only, not as a sensible demonstration. He adduced certain circumstances, and quoted various anatomical facts, which made a continuous transit of the blood from the arteries into the veins, from the veins into the arteries, a necessary consequence; but he never saw this transit; his idea of the way in which it was accomplished was even defective; he had no notion of the one order of sanguiferous vessels ending by uninterrupted continuity,

or by an intermediate vascular network in the other order."

It was only when Malpighi brought the microscope into play that the visible demonstration was perfect, or at least completed. What Malpighi saw in the frog's foot, Leeuwenhock saw afterwards in a tadpole, a bat's wing, and a fish's tail. When coloured fluids were injected in the dead body, another form of demonstration was given.

Harvey's treatise, "De motu cordis et sanguinis circulo," beautifully systematized all that was known at his time, and his experiments demonstrate some points, but to describe the discovery as due to vivisection is an error. It is not possible to ascertain the circulation or to see it in its entirety in the living body. The very act of vivisection renders the demonstration impossible, and the discovery is due to observation of the dead body, not to experiment on the living. We shall continue to hear Harvey's name cited by vivisectors, but his own testimony is that he was first led to the discovery by anatomical observation, and by reasoning therefrom.*

Next to the circulation of the blood, the discovery of the distinct offices of the anterior and posterior roots of the spinal nerves, and the columns from which they arise, is the favourite instance of the results of vivisection. It is strange how vivisectors insist on a claim which Sir Charles Bell has himself denied and repudiated. His express statements as to the purely anatomical source of his discovery have already been quoted. I have lately conversed on the subject with Mr Shaw, Sir Charles Bell's friend and relative, and the able editor and expositor of his published researches. Mr.

* If this discovery were really of such measureless importance (or rather the part due to Harvey), we must look to the middle of the seventeenth century for the advent of a new era in the resources and the success of the practitioners of the healing art. We might expect to find from that date the death-rate wonderfully lessened, and life wonderfully prolonged. Was it so?

Shaw tells me that Sir Charles invariably spoke of his discovery as due to anatomical investigation; that his experiments were performed with the utmost reluctance, and were considered by him unnecessary; and that he often referred to the uselessness and cruelty of experiments on living animals. This is quite in accordance with the humane spirit that appears in all the writings of Sir Charles Bell.

The use of anæsthetics is also often cited as an instance of the benefit of experiments on animals. "Surely any amount of suffering that the case might have required might have been legitimately inflicted upon the lower animals, to secure such an inestimable boon to humanity." These are the words of Dr. Carpenter, a humane man as well as a distinguished physiologist, and who, when a lecturer on physiology, never exhibited experiments on living animals to his pupils. Dr. Carpenter, it will be observed, puts the case hypothetically—*might have been* legitimately inflicted. He knew that ether, and chloroform, and the anæsthetic uses of them, were not discovered by experimenting on living animals, in the sense that vivisectors wish the statement to be understood. The fact is, that the use of chloroform was the result of an experiment, and rather a perilous one, tried by Sir James Simpson upon himself, and by his assistant, Dr. Keith, as they have graphically narrated. The previous use of ether as an anæsthetic was also the result of experiments upon himself by an American dentist. Many experiments have since been performed on animals; but the reference to anæsthetics, as an argument for vivisection, is an unworthy appeal to popular ignorance of the real state of the case.

Not less futile is the claim made as to the discovery of vaccination being due to experiments on living animals. It is well known that the discovery was made by Dr. Jenner from observation. He observed that many of the people in the dairy district of Gloucester enjoyed a remarkable im-

munity from small-pox. On making inquiries he observed that cows had occasionally a pustular eruption on the udder, and that those who milked them contracted similar pustular disease on their hands. He observed that such persons enjoyed sure immunity from small-pox. He ascertained that this was the general and long-known experience of the country people. They had not reasoned on the subject, but they had observed the facts, which Dr. Jenner now observed, and in consequence of which he carried on the inquiry, guided by his superior knowledge and judgment. He observed that those cows which had their udders affected had been milked by persons who had been handling horses with the affection known as "grease in the hoof." The two facts, ascertained by pure observation, were, that certain persons enjoyed immunity from small-pox, and that this immunity was due to the action on the system of another virus derived from a pustular affection in the lower animals. These observed facts really formed the basis of that discovery, which has been of such incalculable benefit to the human race. The inoculation of a boy with this animal virus, instead of with the small-pox matter as then done, supplied a crucial instance and crowning test of the success of the theory. Here is Jenner's own account of this case:—

"During the investigation of the casual small-pox I was struck with the idea that it might be practicable to propagate the disease by inoculation, after the manner of the small-pox—first from the cow, and finally from one human being to another. I anxiously waited some time for an opportunity of putting this theory to the test. At length the period arrived. The first experiment was made upon a lad by the name of Phipps, in the spring of the year 1796, in whose arm a little of the vaccine virus was inserted, taken from the hand of a young woman, who had been accidentally infected by a cow. Notwithstanding the resemblance which the pustula thus excited in the boy's arm

bore to variolous inoculation, yet, as the indisposition attending it was barely perceptible, I could scarcely persuade myself that the patient was secure from the small-pox. However, on his being inoculated some months afterwards, it proved that he was secure. This case inspired me with confidence; and, as soon as I could again furnish myself with virus from the cow, I made an arrangement for a series of inoculations. A number of children were inoculated in succession, one from the other; and after several months had elapsed, they were exposed to the infection of small-pox, some by inoculation, others by variolous effluvia, and some in both ways, but they all resisted it."

Let it be remarked here that the discovery was made, and the demonstration completed, so that the medical profession adopted the practice of vaccination, and the whole civilised world recognised its importance and value, before a single experiment had been made upon a living animal. A few experiments were afterwards made, not by Jenner, such as inoculating a cow with the virus from the heel of a horse; but this was not necessary to prove the efficacy of vaccination in protecting the system from small-pox. It may be said, also, that the inoculation of Phipps and the other patients were really experiments, and might have first been performed on other animals without risking human life. But experiments on lower animals, in this as in other researches, although giving some ground for reasoning by analogy, could not be accepted as conclusive. Trial of vaccination, and of subsequent exposure to small-pox infection, *must*, after any number of experiments, have been made in actual practice.

The discovery of vaccination by Jenner, and its adoption by the profession, can by no stretch of sophistry be twisted into a fair defence of vivisection. Yet we find Sir William Gull saying before the Royal Commission on Vivisection (5529, evidence):—"The whole theory of vaccination came from experiments on living animals." We cannot for a

moment imagine that Sir William Gull was purposely misleading the Commissioners. The fact of a statement so unfounded being made by a man so eminent as Sir William Gull, proves how little accurate knowledge exists of the history of those discoveries on which vivisection rests its claims. Bold assertions are made, and repeated, till those not familiar with the subject receive them as true. Denials and refutations have no chance of equal attention. The public press proclaims and spreads abroad these statements, but refuses admission to counter-statements, and to arguments in disproof of the claims of vivisection.

"What has vivisection done for humanity?" This is the title of an article which appeared in the *British Medical Journal,* the organ of the British Medical Association, in January, 1875. It was at the time when there was considerable agitation, both within and beyond the profession, in consequence of the prosecution of some medical men at Norwich, at the instance of the Society for the Prevention of Cruelty to Animals. The case attracted much public notice, and the report of the proceedings has been published in detail. It is not necessary to refer to it here, except briefly to remind my readers of the circumstances of the trial which led, as will be seen, to events of great public importance.

At the meeting of the British Medical Association at Norwich, in 1874, M. Magnan, a French physiologist of high repute, was invited or offered to exhibit on live animals some experiments demonstrating the effects of alcohol on the system. Dogs were fastened down to the operating tables by their heads and legs, and then, through tubes inserted into their thighs, absinthe and other alcoholic fluids were injected. The operator was assisted by several medical practitioners of Norwich, and there were numerous spectators.

An eminent London surgeon was nominated as arbitrator,

and allowed the experiments to continue; acting, as we are willing to believe, against his better feeling and judgment, with a desire not to seem to oppose the principle of experimenting upon living animals, rather than with direct approval of this particular series of demonstrations.

The cruel proceedings did not, however, go on without protest from some who were present. Mr. Tuffnell, President of the College of Surgeons of Dublin, loudly expressed his indignation at what he witnessed, and during one of the operations cut the tapes by which the poor victim was bound, and setting it at liberty left the place in disgust. On his way out of the house he also set free a number of cats which were shut up in a room waiting for being experimented on. The great majority remained to see the experiments.

The Royal Society for the Prevention of Cruelty to Animals very properly instituted proceedings against the Norwich medical men who assisted at the operations, M. Magnan being beyond reach of prosecution. At the trial, witnesses described the "groaning" of the dogs, their "writhing agony," and in one of them, "epileptic convulsions," all which made what was well called a "ghastly scene." Sir William Ferguson, being asked at the trial his opinion, condemned the whole exhibition as a wanton piece of cruelty. The general effects of alcohol on the system are well known; and special points, indicated by M. Magnan, could be observed in ordinary practice far more certainly than by experiments under unnatural conditions. The Norwich magistrates agreed in the opinion that the experiments were cruel and useless; but eventually dismissed the case, as the offence did not seem to come within the meaning of the Act under which the prosecution was laid.

When the report of the trial appeared in the newspapers, and was widely circulated as a pamphlet by the Prevention of Cruelty Society, public opinion was deeply

moved; and the agitation increased, till parliamentary enquiry was demanded, ending in the appointment of the Royal Commission.

The professional vivisectors and their friends naturally felt alarmed at the agitation. If other cases were brought before English magistrates and English juries the results of the trials might be inconvenient, and would certainly be discreditable. The influence of the medical profession was therefore invoked to shelter the vivisectors from prosecution. By active efforts, both with the Government and the public press, the anti-vivisection movement was, as far as possible, countermined; and on the appointment of the Royal Commission, two members favourable to vivisection were nominated, while scientific or medical opponents of the system were unrepresented. The influence of the General Medical Council, and of various representative bodies and eminent men in the profession, was exerted to neutralise the popular feeling against the system.

The majority of the profession acquiesced in the proceedings of the scientific defenders of vivisection, and resented the popular agitation, which was made to appear as if it were the result of ignorant and fanatical opposition to scientific research. The protest of those medical men who knew the real merits of the question was overborne. The moderate measures of Lord Hartismere and of Dr. Lyon Playfair were scouted, and the influence of the General Medical Council and the medical press, of which Mr. Erichsen and Professor Huxley were the representatives in the Royal Commission, directed the conduct of the inquiry, and led to the Report upon which the Legislature passed the present Vivisection Act.

The physiological laboratories are now protected from popular interference, and experimenters delivered from fear of prosecution under Acts previously in force. The only hope now rests in the return of the profession to the sounder scientific views which prevailed before the Con-

tinental ideas of physiological study and education found favour in England.

The Academy of Sciences at Paris, at the annual meeting, after the Norwich trial, testified its approval of M. Magnan's researches by awarding him a prize of 2500 francs. The opinion of the medical profession in England has been divided as to the acquittal of the Norwich experimenters, the majority, perhaps, approving, but a large number sharing the feeling that such experiments were not demanded in the interests of science. In order to strengthen the feeling in favour of vivisection the article in the *British Medical Journal* was prepared, to which the attention of the reader is now invited. In it we may be sure that the strongest case is put in defence of the system, and chiefly on the point of the alleged necessity of vivisection for the advancement of physiology.

The following list of discoveries is given as being due to vivisection:—

1. Discovery of the two classes of nerves, sensory and motor, by Sir Charles Bell.

2. Discovery of the functions (motor) of the seventh pair, by Sir Charles Bell. Previously to this discovery, the *portio dura* was often cut by surgeons for the cure of neuralgia.

3. Discovery of the functions of the anterior and posterior roots of the spinal nerves by Sir Charles Bell.

4. Discovery of the functions of the anterior and posterior columns of the spinal cord by Brown-Séqnard, and others.

5. Discovery of one of the functions of the cerebellum in co-ordinating muscular movements by Fleurens, and others.

6. Discovery of the functions of the grey matter on the surface of the cerebral hemispheres, as connected with sensation and volition, by Fleurens, Magendie, and others.

7. Discovery of the motor functions of the grey matter covering certain convolutions in the anterior part of the

cerebral hemispheres, by Hitiz, Fritsch, Ferrier, Gudden and Nothnagel.

8. Demonstration of the circulation of the blood, by Harvey.

9. Measurement of the static force of the heart, and discovery of other hydraulic phenomena of the circulation, by Stephen Hales, Ludwig, &c.

10. Discovery that atmospheric air is necessary to the maintenance of life, and that when stupified by its withdrawal, animals may be resuscitated by readmitting it, by Robert Boyle, 1670.

11. Discovery that atmospheric air by continued breathing becomes vitiated and unfit for respiration, by Boyle.

12. Discovery that the air was not only vitiated but also diminished in volume by the respiration of animals, by Mayou, 1674.

13. Discovery of the relation, as regards respiration between animal and vegetable life, by Priestley.

14. Great discoveries, by Lavoisier, on the physiology of respiration, from 1775 to 1780; namely, that oxygen is the vital element of the air, and that animals confined die when oxygen is absorbed or converted into carbonic acid, nitrogen being entirely passive.

15. Numerous facts in the physiology of digestion, observed by Blondlot, Schwann, Bernard, Lehmann, and others.

16. The discovery of the functions of the lacteals, by Colin, Bernard, Ludwig, and others.

17. The discovery of the functions of the eighth pair of nerves in relation to deglutition, phonation, respiration, and cardiac action, by John Reid and others.

18. The discovery of the functions of the sympathetic system of nerves, by Pourfour de Petit, in 1727; Brachet, in 1837; John Reid, and Brown-Séqnard.

19. The discovery of the phenomena of diastaltic or reflex action by Dr. Marshall Hall.

20. The discovery of the action of light on the retina, by Homgreen, Dewar, and M'Kendrick.

21. The discovery of the glycogenic function of the liver, by Bernard, Macdonnell, and Pavy.

22. The discovery of the whole series of facts in the domain of electro-physiology, by Matteucci, Du Bois-Raymond, Pflüger, and many others.

It appears from the evidence before the Royal Commission that the article adopted as a leader in the *British Medical Journal* was prepared by Dr. J. G. M'Kendrick, Lecturer on Physiology, at Edinburgh. In reply to a question by the Commissioners (3878) as to what he thought vivisection had done for humanity, Dr M'Kendrick referred to that published article, adding, "All of the facts which were discovered by these investigations now form, as it were, the groundwork of the knowledge of all medical men in the detection and treatment of disease." At the request of Lord Cardwell, the Chairman of the Commission, the paper in the *Medical Journal* was put in, and is reprinted in the evidence (3916).

Being asked if there had been any criticisms on the paper, Dr. M'Kendrick said—"I have not seen them myself in any journal; some one told me that he had seen a criticism or some observation about it somewhere, but I have no distinct recollection of it. I certainly did not see it" (3940).

Whether any criticism has since appeared I am not aware, but it is certainly not from the document being unanswerable, as a very brief examination of it will show.

On being asked if the list of 22 instances of the benefits derived to human beings, through the advancement of the knowledge of physiology by means of vivisection, include the whole number, the reply is: "No. I think that I have mentioned the most important which I can remember. I prepared that list with very great care at the time, and none besides occur to me at this moment.

Now, let us analyse this very carefully prepared list of discoveries alleged to be due to vivisection.

The first four refer to the discovery of the two classes of nerves, sensory and motor, and the functions of the anterior and posterior column of the spinal cord, by Sir Charles Bell and by Dr. Brown-Séqnard and others.

Here is the old and reiterated assertion, as to experiment being the source of what was really due to observation. Dr. M'Kendrick, in his evidence, enforced the assertion by an illustration. If, for instance, a man was paralysed on one side of the body, how could we tell that the paralysis was due to affection on the opposite side of the brain, without knowing that the fibres in the spinal cord cross over at the upper part of the cord to the opposite side of the brain? Lord Cardwell very shrewdly remarked that "One would have supposed that the crossing of the fibres might have been discovered by anatomy" (3879). The answer was, "The practical fact is, that it is extremely difficult, I should say almost impossible, to trace accurately the course of the fibres in the softer parts of the central nervous system."

Could there be a more unsatisfactory tone of reply? The decussation is manifest to the naked eye, and can be traced by the anatomist even in the softest parts when prepared for examination. Sir Charles Bell himself never made this objection. The decussations have been made still more clear by sections for microscopical observation. The examination of the dead body, in cases where symptoms had been carefully observed during disease, has supplied far more useful and trustworthy facts for diagnosis and treatment than all the experiments made by physiologists on living animals.

The same remark applies to all the alleged improvements from experiments on the functions of various parts of the nervous system, including those numbered 5, 6, 7, 17, 18, in Dr. M'Kendrick's list. It is quite true that many facts and phenomena have been very conclusively shown by means of

experiment, but it is not proved that observation, whether in the living or the dead body, could not afford sufficient knowledge for guidance either in the preservation of health or the treatment of disease. I maintain the sufficiency of facts obtained by observation, even in the practical uses of the alleged discovery of diastaltic or reflex action by the experiments of Dr. Marshall Hall. These experiments are constantly appealed to, especially in arguments for vivisection addressed to the profession. For medical men are quite as liable as the outside public to be led away by strong and reiterated assertion, in matters about which they have not leisure for personal and careful examination.

Dr. Marshall Hall's discovery of reflex action, it is said, has led to great improvements in the treatment of epilepsy and other nervous diseases; he discovered reflex action by experiments; therefore we must stand up for vivisection against ignorant fanatical clamour! These are the very words with which a medical man, better known, however, as a naturalist than a practitioner, answered my inquiry as to what he thought of vivisection. It was no use trying to argue the matter with him in a passing talk. The physiologists say experiments have revolutionised medical knowledge and practice, and Marshall Hall's discovery alone is sufficient to establish their position. Is it? Let us see.

Reflex actions are those arising from the spinal cord, independent of the brain, induced by impressions on the branches of nerves, even when severed from any connexion with the brain. For instance, when Mr. Bouillaud, in one of his experiments, had destroyed the cerebral lobes of a dog by red hot irons, so that there were no longer intelligent movements, still he found that the animal shrank when cold water was dashed at it, and withdrew its feet when they were pinched. Sir George Burrows gave a less repulsive example to the Royal Commissioners (186). A man in hospital is supposed to be paralysed; the nurse tells the doctor that he must be feigning, for she saw him move his

legs in the night. On being asked to move his legs, he remains motionless, and is evidently unable, though making effort of will to do so. "But if you uncover the bedclothes, and just touch the fellow's foot with a feather, he will draw his legs up, and not know that he is doing it. That is from an independent function in the spinal cord." This very simple experiment of Sir George Burrows is quite as decisive as that of M. Bouillaud, or the very horrible experiment on "recurrent sensibility" described in the Handbook for the Physiological Laboratory.

The truth is, that no experiments at all are needed for demonstrating the processes of reflex action, nor do they help towards applying the knowledge to practice, although this assertion is made. So far from leading to improved treatment of epilepsy, or other diseases supposed to be chiefly dependent on the spinal cord, the ill-digested knowledge of what Marshall Hall really did and taught has led to stupid routine, and contracted views of maladies which require most intelligent and varied treatment. This depends, in every individual case, upon conditions only to be ascertained by careful observation, or what Marshall Hall himself calls "living pathology." Apart from his experiments, no medical writer gives more shrewd and instructive remarks on the diagnosis and treatment of epileptic and other nervous diseases; but these are overlooked in the anxiety to quote his experiments in support of vivisection. It is not the multiplication of details about the nervous system that is wanted, but the wise interpretation of facts and phenomena already familiar.

So much has been said about reflex action that I have dwelt longer on the point than there was really occasion. With regard to other therapeutic or practical benefits, connected with or said to have arisen from experiments on living animals, the only one calling for distinct notice is "the abandonment of the operation of cutting the fifth pair for neuralgia." If this was often practised one would

suppose that the inefficiency of the remedy would be ascertained by a few operations.* But this statement is intended to convey the idea of numbers of people remaining with distracting pain and distorted faces, till vivisectors advised surgeons to abandon the operation!

With regard to the alleged discovery of the functions of the several parts of the encephalon, to the experimental investigations of which some hundreds of physiologists have devoted much labour, there are very few results universally accepted. If we include articles and reports in medical and scientific journals, as well as treatises separately published, we have a huge library describing such investigations, but the conclusions arrived at would not fill one octavo page. There is not a subject in the whole range of research about which there are so many vague and so many contradictory statements. The most recent experimenters seem to be going over the same dreary and dismal ground as their predecessors. Very few who are not specialists in physiological literature can verify this assertion, which I make after comparing the reports of contemporary vivisectors, with those of Longet, Bouillaud, Legallois, Magendie, and Fleurens. In fact, some of the earlier physiologists, especially Tiedemann and Serres, can show results far more worthy of attention than the modern vivisectors of France and Germany, with all the superior advantages these possess in the use of anæsthetics, and in the appliances of laboratories, such as those of Ludwig, of Leipzig; Müller, of Berlin; and Pflüger, of Bonn. The earlier vivisectors gave due prominence to results obtained from pathology and from comparative anatomy, and did not maintain, like our modern physiologists of the vivisection school, that "the whole knowledge of the animal system is derived from experiments on living animals."

* "*Experience has proved* that the relief, if any, is but partial and temporary, and that the operation may, in fact, be the means of converting simple neuralgia into irremediable structural disease."—*Miller's "Surgery."*

This was said in evidence repeatedly, with slight variation of phrase, by the advocates of vivisection before the Royal Commission. So far from such being the case, the remark of Bowman, in his standard work on Physiology, commends itself to every unbiassed mind as true, "Vivisections upon so complex an organ as the brain are ill-calculated to lead to useful or satisfactory results." This is the same conclusion at which Dr. Pritchard arrived when he said that "the results obtained by experiments not only differ from each other in essential respects, but are completely opposed to those deduced from the minute and accurate observation of pathological facts."

The next discoveries (8, 9) include the circulation of the blood, and the various researches as to the force of the heart, the velocity of the blood, and kindred subjects. Of Harvey's discovery, and the proportion borne by vivisection in it, enough has been said. As to the experiments on the statics and dynamics of the circulation, from those of Hales to those of Ludwig, no doubt many facts have been ascertained and recorded, as is the case with all experiments, but no new or practical results appear "for the benefit of humanity." As to the absolute force of the heart considered as a hydraulic machine, and the velocity of the blood, the results of experiment vary much, and those of old Stephen Hales give probably as near an average estimate as can be expected. But for practical application in medicine the numerous experiments made since the time of Hales are quite useless. The force of the heart, for example, varies in the animals inspected, and under different conditions; and the variations are infinite in different persons, in various conditions of age, strength, and state of health. The general estimates may be interesting as facts for philosophical statement, but are useless with any view of applying such experiments to use, in maladies either of the sanguineous or nervous system. More useful information can be obtained by observing the force of the heart as indicated on the delicate

dial of a balance chair, than from all the experiments of vivisectors.

From numbers 10 to 14 of Dr. M'Kendrick's list, the discoveries ascribed to vivisection need only to be named to show how futile are the claims. No painful experiments on animals were required to prove that atmospheric air is necessary for the maintenance of life; nor that atmospheric air, by continued breathing, becomes vitiated and unfit for respiration; nor that it is diminished in volume by respiration; nor to show the relation of animal and vegetable life in regard to the condition of the atmosphere. All these discoveries belong to chemistry, and were ascertained and proved by facts and occurrences in common life, and observed in ordinary course of scientific investigation. The sad tragedy of the Black Hole at Calcutta, and the frequent calamities from "choke damp" in mines, proved the effects of vitiated air, without the stupid demonstration of throwing dogs into the *grotte del cane*, far less of "experiments" by physiologists. When the interpretation of these facts was given, by the discoveries of Priestley and Lavoisier, it was a triumph of chemical, not of physiological science, and entirely apart from vivisection.

The physiology of digestion comes next (15). Numerous experiments have been made by Schwann, Bernard, and other vivisectors; but all the facts demonstrated by them and many more, could have been ascertained by simple observation, without vivisections. If the French physiologists had taken the trouble to attend at the Parisian Abattoirs, they could have "experimented" and made observations on animals necessarily doomed to death, without injuring and destroying needless victims in their laboratories. And even in the living human subject opportunities have occurred of ascertaining all the processes of digestion, for which such cruel vivisection of animals has been performed. By such experiments, in unnatural conditions of animals, no practical or useful light can be thrown on the

natural processes of human digestion, in all its varieties and idiosyncracies. Abernethy, from observation and experience, knew more about digestion, and used his knowledge for the benefit of humanity, more successfully than all the vivisectors, whose practices he opposed and denounced.

With regard to the function of the lacteals a few careful and well-directed observations, at the Abattoirs, or at any ordinary butcher's slaughter-house, would have served the same purpose as all the experiments needlessly performed in the laboratories of Colin, Bernard, and Ludwig. Anatomy had long before shown the structure and course of these vessels, and their use in regard to nutrition was well known to physiologists. In the Museum of the Royal College of Surgeons, of London, there is a remarkable series of preparations, exhibiting to anatomists the lacteals and the lymphatic vessels, and the absorbent vessels of the digestive system, injected with size, and vermilion, and mercury. No new demonstrations were needed for anatomical knowledge; and no new experiments were needed for the advancement of medical practice. Yet this is one of three notable instances which the Royal Commissioners, in their Report, describes as having been "selected for them" by Professor Turner, of Edinburgh, "in illustration of the extent to which practical medicine has been improved by physiological experiment" (p. 13).

The other two notable instances are, "the discovery of the circulation of the blood" and "Sir Charles Bell's discovery of the compound function of the spinal nerves." How far vivisection was necessary for these the reader is now prepared to judge. It is well that, in another part of their official Report, the Commissioners say, "We have not thought it part of our duty, the majority of us not having had professional training, to decide upon matters of differing professional opinion, but we have been much struck by the consideration that severe experiments have been engaged in for the purpose of establishing results which have been

considered inadequate to justify that severity, by persons of very competent authority. Cases may not improbably arise, in future, in which the physiologist may be disposed to underrate the pain inflicted in the course of establishing results which may prove to be trivial or even worthless." Dr. Samuel Johnson may be regarded as not a person of competent authority, but it is curious that he refers to this very instance of the functions of the lacteals in his celebrated paper against vivisection (*Idler*, No. 17). "I know not that by living dissections any discovery has been made, by which a single malady is more easily cured. And if the knowledge of physiology has been somewhat increased, he surely buys knowledge dear who learns the use of the lacteals at the expense of his own humanity. It is time that universal resentment should arise against those horrid operations, which tend to harden the heart and make the physician more dreadful than the gout or the stone." Dr. Johnson was, at that time, the friend and associate of the highest men in the medical profession, and would not have thus written if they approved of vivisection.

The discovery of the action of light on the retina (20) we might naturally expect to find in Dr. M'Kendrick's list, having himself contributed some experiments for its illustration. But there are few who would admit that practical knowledge on this subject depended on vivisection, any more than "the discoveries of the whole series of facts in the domain of electro-physiology." These discoveries may have "important practical bearings," but the principles on which the practice rests were the result of scientific observation, and of researches in which vivisection gave no essential aid (22).

The only remaining discovery is that of the glycogenic function of the liver (21). This has been much vaunted as an important contribution to physiological knowledge, applicable to improvement in medical practice. Mr. Erichsen, as spokesman of the Commissioners, made the most

of it in taking the evidence of Professor Turner. "In diabetes it was supposed, not many years ago, that the sugar was formed in the kidneys; it is now known by physiological experiment that the sugar may be produced by a lesion of the nervous system. Claude Bernard has shown that, if a certain portion of the brain is injured, you get sugar in the urine; that the sugar has nothing more to do with the kidney, and is no more a kidney disease, in point of fact, than the purulent expectoration in a consumptive patient has to do with the mouth; that the kidney merely evolves it from the system, just as the mouth ejects the purulent matter from the lungs?" To which Professor Turner replied, "That is the case" (3126). Mr. Erichsen's question was evidently framed for the instruction of his non-professional colleagues of the Commission.

The analogy suggested between the expulsion of diabetic sugar by the kidney and of purulent sputa by the mouth was rather a strong figure of speech; but, passing this, it was scarcely right of Mr. Erichsen and Mr. Turner to make the Commissioners suppose that "not many years ago sugar was believed to be formed in the kidneys." As long ago as the time of Dr. Mead, that distinguished physician ascribed the diabetic urine to a morbid state of the liver and bile. A century ago Dr. Cullen taught that the morbid state of the urine arose from disorder of the nutritive and assimilative functions connected with the digestive system. This was received by the profession generally; and melituria was understood to indicate an abnormal result of animal chemistry, one process of which, in natural health, was the production of sugar. What Bernard showed was, that the formation of sugar in the liver in the normal state is so constant that the liver may be regarded as the sugar-producing organ. He demonstrated this by numerous observations, especially by examining the livers of seven recently-dead human subjects. Five of these

were executed criminals. In three healthy livers he determined the absolute weight of sugar, finding an average of 22.03 grammes; while, in the liver of a diabetic subject, where death was sudden, from pulmonary apoplexy, the amount of sugar was 57.50, or more than double. The glycogenic function of the liver was thus demonstrated in a legitimate way; and in the Abattoirs he could have performed any number of *post-mortem* experiments, if confirmation or further elucidation were desired.

But, unhappily, Bernard showed the way to experimenting on living animals. He found that, by pricking or piercing the floor of the fourth ventricle of the brain, he could increase the saccharine secretion in the liver. In this line of experimentation he has been followed by many physiologists, especially by Brunton, Pavy, Ferrier, and Schiff. In reviewing these experiments, many of which have been painful and destructive of life, I find most confused and variable results. Those results which seem the most certain are such as might be anticipated from the slightest consideration of physiological principles. Thus, it is announced that the activity of the glycogenic function is increased with an augmented flow of blood to the liver, such as takes place a few hours after a meal. On the other hand, when animals were starved—as by Dr. Brunton with rabbits, or by Dr. Wickham Legg by tying the bile ducts of cats—no irritation of the fourth ventricle will cause glycogen to appear in the liver or the urine. Schiff produced diabetes by division of the anterior columns of the spinal cord. Dr. Pavy had the same result, by dividing the superior cervical ganglion of the great sympathetic; but this lesion also caused inflammation of the lung, or pleurisy, so that the animals could not be observed for long periods, as Schiff in some cases did.

Now, all these experiments go no further than to show that the normal secretion of sugar depends on healthy action of the organs engaged in nutrition; while unnatural

interference with the actions of these organs, especially by lesion of the nervous centres by which their action is sustained, produces abnormal secretion of sugar, and diabetes. This multitude of experiments I regard as unjustifiable and needless cruelties, and leading to no useful result.

Having thus examined all the 22 alleged discoveries, claimed as due to experimental research, I must leave the reader to judge whether vivisection is "the main source of our knowledge of physiology."

A separate list is given of results "in aid of medicine." If the testimony of physicians of the highest rank in the profession is accepted, their verdict is against the alleged benefits of vivisection in the practice of the healing art. Sir James Y. Simpson did not even allude to it, in enumerating the causes of the advancement of medicine and surgery during the last half century. Professor Newman says: "I can attest that Dr. James Cowles Prichard assured me that vivisection had added nothing whatever to the physician's power of healing." When Sir Thomas Watson was giving evidence before the Royal Commission, the question was asked: "Although you have never performed any experiments, nor witnessed them, you have used the results of the experiments of others, have you not, as the basis for the advancement of your professional knowledge?" The answer was: "I have made myself acquainted with the experiments and their results, and have turned them to such uses as I could." Of this reply Mr. Macilwain, himself a distinguished and experienced surgeon, in reviewing the evidence, says: "Could any answer convey a more measured recognition of a mode of study, in reply to the question whether he had not made it a *basis* for the advancement of his professional knowledge? Could anything be more vague or unsatisfactory? Why was so experienced a witness not requested to favour the Commission with some of the details of so vast an experience?

Why was he not requested to state in *what* cases he had turned it to accouut, and how far it had or had not answered his expectations?"

The truth is, that the question was put apparently for the sake of the lay members of the Commission, and for the non-professional readers of the Blue Book. It was intended to suggest that vivisection had been the source of improvements, if not of an entire reform of practice, in thus speaking of it as *the basis of advance in professional knowledge.* The interrogator knew too well, however, how imprudent it would be to follow up the tentative question. To have asked for details or examples would have exposed the futility of the claims of the vivisectionists to have amended or altered medical practice. Where attempts have been made to give details, the examples are not only few, but they lead at once back to the very matter under dispute, whether the knowledge on which the practice rests came from vivisection or from legitimate methods of research.

On the article in the *British Medical Journal,* already quoted, entitled "What has Vivisection done for Humanity?" the following are the examples given, under the head of benefits, in "advancing therapeutics, relief of pain," &c.:— 1. Use of ether. 2. Use of chloroform. 3. Chloral discovered experimentally by Liebrich. 4. The action of all remedies are only definitely ascertained by experiments on animals. 5. Action of Calabar Bean by Fraser. 6. Antagonism between active substances and the study of antidotes.—Many observers.

Could there be a more meagre and more misleading set of examples? The practical use of anæsthetics would have been introduced and perfected if a single experiment on an inferior animal had never been made. The action of remedies on the human body can only be definitely ascertained by observation, and experiments on animals are more likely to mislead than to assist in gaining this

definite knowledge. The action of some substances, such as antimony on horses and mercury on dogs, is widely different from their action on the human subject; and the effects, both of remedies and of poisons, vary much in the different animals experimented on. Dr. Thorowgood says he has seen opium given to a pigeon, enough to kill a strong man, without any effect. Goats have been known to browse on tobacco-leaves, and rabbits on belladonna, without harm. Many such anomalies have been observed, and the only certain knowledge of the influence of substances on the human subject must be obtained by observation of cases in private or in hospital practice.

In the debate on Lord Truro's "Cruelty to Animals" Bill, in the House of Lords, Earl Beauchamp adduced the fact that each year 20,000 human beings lost their lives from snake-bites, and asked if a cure for snake-bites would not be a discovery of vast importance. He intended to convey to their lordships the idea that vivisection can make this discovery. Multitudes of experiments have already been made without result. Even if an antidote should appear to have some influence on the animal operated upon, the result might be different in the human subject. The only possible way of testing alleged antidotes —and the natives of different regions profess to know this—is to apply them in actual cases of snake-bite. For such there must be frequent opportunity, if 20,000 cases occur yearly. These are experiments which can do no harm, and might lead to discovery of cure. The poisoning of animals in order to try possible remedies is a needless system of cruel experiment.

The "action of the Calabar Bean," the only distinct example specified, is no argument to adduce in such a discussion. It was reported to be a very dangerous poison and Sir Robert Christison determined to try its effect upon himself—a very fair "experiment on a living animal;" as was that of Sir James Simpson and Dr. Keith in testing

the effect of chloroform as an anæsthetic. Of course, Sir Robert Christison proceeded with extreme caution, and apportioned the dose with much care, finding the effects such as had been reported by the missionaries in Africa. He then remitted the further examination to his assistant, Dr. Fraser, who, in course of experiments, noticed the remarkable effects of the bean on the pupil. With due caution, as in Sir Robert Christison's case, this effect might have been more certainly and directly observed in the human subject, and with no more danger or inconvenience than with other poisonous substances which, in minute quantities, are used as medicines. At all events, it is trifling with the question to single out this physiological fact as an example of the improvements in medical practice due to vivisection! A stronger example would have been the action of Laburnum Bark. In a case of suspected poisoning with this substance, some trials by Christison on animals were thought necessary for the satisfaction of the jury, just as the performance of vivisections was undertaken by Sir Charles Bell for the satisfaction of the Council of the Royal Society.

Passing from poisons, in regard to which some of the most plausible apologies for experiments have been urged, other pleas put forth in the *British Medical Journal* are scarcely worthy of reply or refutation. It is said, for instance, (page 56, Jan. 9, 1875), "Without vivisection-experiments, we would know almost nothing of the phenomena of inflammation." After all the observations of physicians and surgeons, of physiologists and pathologists, for successive generations, at home and abroad, we are told to look to vivisectors for almost all our knowledge of the causes, the symptoms, and the results of inflammation! The plea is preposterous, and the fact of it being seriously put forward in an article specially written in defence of vivisection is sufficient to show the groundlessness of the alleged practical benefits of this method of research.

The article concludes with the following sentences, the mere quotation of which, will suffice to show the inordinate claims and pretensions of vivisection:—"To record all the facts given to physiology by experiments on animals would simply be to write the history of the science. Therapeutics is yet in its infancy; but *nearly all the facts definitely known regarding the actions of remedies have been gained by experiments on animals!* To stop experiments on animals would as surely arrest the progress of physiology, pathology, and therapeutics, as an edict preventing the chemist from the use of the retort, test-tube, acids, and alkalies, would arrest the progress of chemistry." On reading this, I wondered what would be the effect of such an assertion in the minds of the intelligent readers of the *British Medical Journal*—of those, at least, outside the circles of vivisectors, and their advocates or apologists. Have all the observations of clinical medicine, of pathological anatomy, of pathological histology, and pathological chemistry been vain and fruitless? Have all the labours recorded in books of medicine and surgery, in medical reports and the transactions of societies, in practical manuals and text-books, and in our official pharmacopœas and dispensatories been delusive and misleading? Almost the whole classic literature of the profession belongs to a time when, in England, the practice of vivisection was comparatively unknown, and when its results were regarded with doubt, if not with condemnation. Have all the generalisations and conclusions of past experience been superseded by the results of this new method of research? Has the healing art, in short, been wholly revolutionised since vivisectors came into the field? The official reports of the Registrar-General, the pages of our medical journals, the case-books of our practitioners, refute the claim. Till some better statement can be given of "what vivisection has done for humanity," respectable medical men will keep to the old paths—paths of honour, and not of shame.

Assuming, for the sake of argument, that such experiments may have been of scientific value, or may have led to the discovery of scientific facts of permanent importance, could such discoveries not have been arrived at by a broad and comprehensive study of natural phenomena, or of those quasi-natural facts, which are the continual accompaniments of civilisation? In short, could not observation have sufficed, without experiment on living animals?

To this I give a direct answer, so far as physiology is concerned, in the words of the great Cuvier: "Nature has supplied the opportunities of learning that which experiments on the living body never could furnish. It presents us, in the different classes of animals, with nearly all possible combinations of organs, and in all proportions. There are none but have some description of organs by which they are made familiar to us; and it only is needful to examine closely the effects produced by these combinations, and the results of their partial or total absence, to deduce very probable conclusions as to the nature and use of each organ, and of each form of organ in man."

Another eminent physiologist, Dr. Carpenter, says, "Almost all our knowledge of the laws of life must be derived from observation only. Experimentation can conduct us very little farther in this inquiry. The ever-varying forms of organized beings by which we are surrounded, and the constantly-changing conditions in which they exist, present us with such numerous and different combinations of causes and effects, that it must be the fault of our mode of study, if we do not arrive at some tolerably definite conclusion as to their mutual relations." Specially, on one branch of experimental research, engaging a large share of attention in physiological laboratories, Dr. Carpenter says: "On such subjects as the functions of the different parts of the encephalon, I do not believe that experiment can give trustworthy results, since violence to one part cannot be put in practice without functional disturbance of the rest. Here

I consider that a careful anatomical examination of the progressively complicated forms of the encephalon from fishes up to man,—*the experiments already prepared by nature*—is far more likely than any number of experiments to elucidate the problem."

No clearer statement could be given as to the value of comparative anatomy and physiology, or observation of the structure and functions of the organs of the lower animals, in the study of human physiology. I may add, that the observation of abnormal specimens of the human body is also capable of affording conclusions which experimenters seek to arrive at by their painful processes. A careful collection and arrangement of such observations would establish, and has established, many facts in physiology with far greater certainty than experiment could do. In truth, the observation of the human organs, in their early development and in cases of anomalous growth, affords many examples of "experiments prepared by nature."

Of the light thrown on physiology by the facts and laws of physical and chemical science, it is needless to speak in detail. Reference is made to these branches of science in this place, only because the advocates of experimental physiology, as we have seen in examining Dr. M'Kendrick's list of alleged discoveries, unfairly adduce facts of natural science in support of their method of research.

If physiology owes much to comparative anatomy, and also to physics, and to chemistry, it owes much more to pathology. Along with pathology is included morbid anatomy, or the *post-mortem* inspection of structure, for investigation of the results of diseased action in life. When the writer was a student at the University of Edinburgh, there was a good deal of discussion about vivisection, then attracting considerable notice, from the experiments of Magendie and other French physiologists. He well remembers Dr. Abercrombie's strongly-expressed opinion about such experiments,

and his advice to depend on clinical and pathological study for the knowledge that could be applied in the practice of medicine. Having had his attention thus early directed to the claims of experiment, as compared with observation, he has ever since watched the progress of vivisection; and a review of the results now, after forty years, confirms the belief that Dr. Abercrombie's opinion and advice were right. And certainly not the least injurious influence of the present rage for experimenting is its tendency to withdraw attention from seeking the advancement of physiology, as well as medicine, through clinical and pathological study.

Not professed biologists and physiologists only, but men in high position as physicians, are echoing the strange and novel assertion, that all our most important knowledge and improved practice is derived from experiments on living animals. The experience of medical practitioners, in all the ages which are now called pre-scientific, is depreciated, and we are told to expect a new epoch in the healing art. But the more I think of it, the more I admire the courage as well as the wisdom of M. Nelaton, the distinguished surgeon, who professes to belong to the "pre-scientific" school, and declares, in opposition to the loud voice of present opinion, that there is no such thing as "scientific medicine," in the sense understood by Bernard and his admirers; and that every source of information is delusive which is not derived from direct observation of the patient.

Our own most distinguished surgeon, the late Sir William Fergusson, made an avowal nearly as emphatic. When asked if experiments had not led to the successful treatment of complaints, or the mitigation of human suffering, replied (Vivisection Blue Book, 1049), "I may, perhaps, speak more confidently regarding surgery than other departments in my own profession; and in surgery I am not aware of any of these experiments on the lower animals having led to the mitigation of pain, or to improvement as

regards surgical details." Being asked about John Hunter's experiments, Sir William Fergusson said, that "Hunter's first experiment, if it might be so called, was done on the human subject; and it was long after he had repeated his operation on the human subject, and others had repeated it that the fashion of tying arteries and experimenting on the lower animals originated or was developed. He had himself in early life performed such experiments, influenced by what others had done, and by the wish to come up to what they had done in such matters; but the more matured judgment of later years would not allow him to repeat what he did in earlier days. Neither was he aware that any very expert operator on the lower animals had made himself thereby an expert operator on the human subject."

Many testimonies of a similar kind could be cited from most eminent physicians and surgeons. The only reason why stronger opposition to vivisection has not been made is from the prevalence of a vague idea that benefits of a practical kind may possibly result from increased knowledge of physiological facts and phenomena. It is forgotten, meanwhile, how all the most important facts capable of being applied in practice are already set down in books on the principles of medicine and surgery.

Even in regard to pure physiology, the study of diseased action has often given the clue to the discovery of the function of organs. Physiology has learned far more from medical practice than medical practice can ever possibly gain from experiments on the lower animals. It was the study of diseases of the brain that gave the key to what knowledge we possess of the functions of the parts of the encephalon. It was by observing that paralysis of one side of the body was associated with certain diseased conditions of the opposite side of the brain, that the singular fact was established as to the right side being governed by the left side of the brain, and the left side by the right side of the brain. It was by the study of diseased conditions

and their results, by observing symptoms, and by noting the pathological appearances, that the functions of the cerebral hemispheres, and of the corpus striatum, and of the optic thalamus, and other parts of the encephalon were ascertained. The wild exploration of structure and functions, under the unnatural conditions of vivisection, is more likely to retard than to expedite the knowledge of the uses and relations of the various parts of the nervous system. In our homes and our hospitals—not in physiological laboratories—we must study the human frame, in health and disease. The records of medical observation and practice contain boundless materials for induction, if the facts were carefully studied, wisely interpreted, and judiciously applied. I know of no instance where the mode of inquiry, by observation of the human system in health and disease, has retarded the dates of alleged discoveries resulting from experiments on animals. Some new discoveries will be claimed in the future as they have been in the past. But these experiments are so liable to fallacy, and in general so contradictory, that they cannot be used as guides to practice, until the facts are ascertained by scientific and professional observation.

In most cases, the experiments can have no bearing on medical practice. Professors Hitzig or Ferrier may anticipate wonderful results from connecting glycogenic function of the liver with violent injury of the brain in dogs; but no rational practitioner would confine his treatment of diabetes to the subduing of some supposed cerebral lesion.

The discovery of antidotes to poisons is the most plausible ground on which the danger of delay in research can be pleaded. So far as this country is concerned, and in the experience of any general practitioner, ninety-nine in every hundred cases of poisoning, and even a larger proportion, are from substances with which we are perfectly familiar, and the antidotes to which are well known. Our practice in all these cases is intelligently guided by facts of

physiology and of chemistry, confirmed by general experience. In very few cases, indeed, are *specific* antidotes known for poisons, and if any are proposed, their efficiency must be proved in actual practice.

Nor is it by experiments on animals that new discoveries are likely to be made, although the claim is urged—vainly, as we have shown—for this origin of the great "discovery" of vaccination. If any parallel discovery is made, in regard to other fatal diseases, it will be by "experiments" on the human body, not on animals. Even for the benefit of animals themselves, and indirectly for the advantage of man as having property in animals, I have great doubt as to such experiments being ever justifiable. The researches of Dr. Klein, under the sanction of Mr. Simon, Medical Officer of the Privy Council, and assisted by grants of public money, I consider wholly unjustifiable. To produce, artificially, such distressing diseases as typhoid fever, or pyemia, in sheep or cattle is a barbarous proceeding. So is the attempt to develope tuberculous disease in dogs. No practical advantage can be gained *by the artificial production of such diseases* in animals, in throwing light either on their nature or their treatment in man.

I do not know any more striking example of the futile results of experimental inquiry than that which was instituted some years ago on suspended animation. The Royal Humane Society had received from Dr. Silvester, and other medical men, various suggestions as to the best mode of treating persons apparently drowned. The Committee referred the proposals to the Royal Medical and Chirurgical Society, with a request for advice. A committee of investigation was appointed by the Royal Medical and Chirurgical Society, consisting of the following members:—C. J. B. Williams, M.D., F.R.S.; C. E. Brown-Séquard M.D., F.R.S.; George Harley, M.D.; W. S. Kirkes, M.D.; H. Hyde Salter, M.D., F.R.S.; J. Burdon-Sanderson, M.D.; W. S. Savory, F.R.S.; and E. H. Sieveking, M.D.

Now, here was a clear and well-defined object of inquiry: the purpose for which it was instituted was practical and beneficent; the investigators were men of science, able and experienced. Here, if anywhere, clear and satisfactory results might be looked for.

In pursuing the inquiry, a large number of experiments were made upon living animals. In the first place, the phenomena of apnæa, in its least complicated form, were investigated—viz., when produced by simply depriving the animal of air. Tracheotomy was performed upon animals fastened down to a table on their backs, and glass tubes inserted, and secured firmly by ligature. Through a tube thus inserted the animal could breathe freely, but the air could be at once and effectively cut off by inserting a tightly-fitting cork into the upper end of the tube. In this way a measure could be obtained of the time when respiration would cease. In order to observe in the same animals the duration of the action of the heart, long pins were inserted through the thoracic walls into some part of the ventricles. The movements of the pin indicated the motion of the heart, after the cardiac sounds had ceased to be audible. The conclusion from many experiments was that, in simple apnæa, the action of the heart continued a considerable time after the respiratory movements had ceased; a fact well known, and needing no cruel experiments to establish it.

In dogs, the average duration of the respiratory movements, after the plugging of the tube, was 4 minutes 5 seconds; the extremes being 3 minutes 30 seconds, and 4 minutes 40 seconds. The average duration of the heart's action, on the other hand, was 7 minutes 11 seconds; the extremes being 6 minutes 40 seconds, and 7 minutes 45 seconds.

Another series of experiments led to the conclusion, that a dog may be deprived of fresh supply of air during a period of 3 minutes 50 seconds, and afterwards recover

without the application of artificial means, but is not likely to recover after being deprived of air for 4 minutes 10 seconds. Experiments were also made in order to measure the force of the respiratory efforts after the plugging of the glass tube.

Hitherto nothing is ascertained except that the action of the heart continues longer than that of the lungs in suspended animation, and that the death struggles in victims of suffocation vary in duration by a few seconds. On proceeding to experiments on drowning, it was found that the time of possible recovery of dogs, after immersion, was only 1 minute 30 seconds, on an average, instead of 4 minutes from simple deprivation of air. "To what is this striking difference due?" the investigators ask. Experiments were made in order to eliminate from the inquiry the element of struggling, also the element of cold, and, lastly, the access of water to the lungs. On this latter point it was found that a dog with the wind-pipe plugged recovered from a longer submersion than a dog without the wind-pipe plugged. The conclusion from the various experiments on immersion was, that the period of death depended mainly on the entrance of water into the lungs. Violent respiratory efforts hastened this fatal result, while the action of chloroform, as diminishing such struggles, retarded death.

Experiments were next made as to the best means of resuscitation, including galvanism, venesection, cold affusion, actual cautery, and other methods; in all the experiments the animals being suffocated in the usual way by plugging their windpipes. None of the proposed methods obtained any support from the experiments; which failed also in giving any conclusion as to the relative value of the various modes of artificial respiration.

In presenting their report to the Royal Humane Society, the Royal Medical and Chirurgical Society were able to recommend no practical suggestions as the result of their

inquiry. Dr. Edward Smith gave due credit for the extent and accuracy of the facts reported, the care with which they had been ascertained, and the pains taken to estimate the influence of disturbing causes. But in reference to the practical object in the appointment of the Committee, the report, he said, failed. The Committee had not proved that any one of their inquiries was applicable to the human subject. They recorded to a second the time when various phenomena occurred in different dogs, some surviving longer than others, and some recovering more rapidly than others. But the time during which different *men* could be immersed and recover could not be proved by experiments on *dogs*, and the Committee had shown that all their plans for the restoration of drowned dogs had failed. Dr. Webster expressed regret that so much suffering had been inflicted, and the lives of so many dogs sacrificed.. He hoped that in future experiments on living animals would be avoided.

On referring to the Reports of the Royal Humane Society, and making inquiry from its officers, I learn that no modifications of the method of restoring suspended animation in persons apparently drowned resulted from the experimental inquiry. Any slight modification of the method originally introduced to the Society by Dr. Silvester has arisen out of *observation on human bodies, and experience in their treatment.*

Are there not fallacies underlying such a method of interrogating Nature which, of necessity, vitiate the results? A clearer and more forcible reply to this question could not be given than in the words of the old Roman physician and writer on medicine, Celsus: "It is alike unprofitable and cruel," he says, "to lay open with the knife living bodies, so that the art which is designed for the protection and relief of suffering is made to inflict injury, and that of the most atrocious nature. Of the things sought for by these cruel practices, some are altogether beyond the reach of human

knowledge, and others could be ascertained without the aid of such wicked methods of research. The appearances and conditions of the parts of a living body thus examined must be very different from what they are in their natural state. If, in the entire and uninjured body, we can often, by external observation, perceive remarkable changes, produced from fear, pain, hunger, weariness, and a thousand other affections, how much greater must be the changes induced by the dreadful incisions and cruel mangling of the dissector, in internal parts whose structure is far more delicate, and which are placed in circumstances altogether unusual." These remarks of Celsus were made in reference to the inspection of the living bodies of human criminals, who were handed over for this purpose to the "physiological laboratories" of the medical school of Alexandria, and probably to other places of study. The objections to such researches, so strongly urged by Celsus, apply with double force to experiments on the lower animals, where the differences of function and of structure must further diminish the chance of light being thrown on the physiology of man in the natural condition.

That observations made by vivisection are of necessity abnormal and liable to fallacy, reason alone might show independently of experience. The sources of error arise not from any contingent cause, but from the very nature of this method of investigation. Nature, when interrogated, reveals only what is her condition at the moment of examination, and hence, although the permanent and unvarying properties of inanimate matter renders the use of experiment of paramount value, the questioning process is more limited, and its results more uncertain, when applied to living and sentient beings. We cannot depend on the accuracy of conclusions respecting the normal functions of parts, if drawn from experiments which only tell what takes place in those unnatural conditions induced by operations. For not only are the ordinary actions of the organs thereby often

deranged or destroyed, but many causes conspire to render still wider the difference between the observed and the natural condition of the subjects operated upon. The deadening of pain during the actual use of the knife and other instruments is only one element in the contrast, although chloroform itself in many cases increases the sources of fallacy and interferes with results. The excitement and terror of the animal must be taken into account; and there is abnormal action even if the body be made insensible and unconscious. "I do not believe," says Professor Carpenter, "that on such subjects as the functions of the different parts of the encephalon, experiments can give trustworthy results; since violence to one part cannot be put in practice without functional disturbance of the rest."

Experience has confirmed these reasonable objections to experiments on living animals as necessarily liable to fallacy. The results obtained by different experimenters are so various, and often so contradictory, that there is scarcely a single position laid down by them that can with confidence be adopted. We find that the most opposite results occur at different times from injury of the same organs; that injury of different organs often produce the same results; and that the same experiments are not followed by the same results in different subjects. The latter remark applies specially to poisons, the effects of which show remarkable variations in different animals. I think that the true value of these experimental researches was rightly estimated by Dr. Pritchard, who, in his work on insanity, says:—"It is well known to all those who have paid attention to the recent progress of physiology that attempts have been made to ascertain the functions of the different parts of the brain and its appendages by removing successively parts of these organs from living animals, and noticing the changes which ensued in their actions when thus mutilated. The most celebrated of these was the series of experiments instituted by M. Fleurens. M. M. Magendie and Serres, and more

lately Fodera and Bouillaud, have occupied themselves with similar researches. The results obtained from these experiments not only differ in essential respects from each other, but are completely opposed to conclusions deduced from inquiries instituted and pursued for several years on a different path. These inquirers are disposed to distrust all the results of vivisection, or experiments performed by cutting away the brains of living animals. The method of research which they have pursued is that of minute and accurate observation of pathological facts."

The following passage occurs in the work of the late Dr. Barclay, the founder of the Museum of the Royal College of Surgeons of Edinburgh, "On the Muscular Motions," p. 298: "In making experiments on live animals, even when the species of respiration is the same as our own, anatomists must often witness phenomena that can be phenomena only of rare occurrence. After considering that the actions of the diaphragm, in ordinary cases, are different from its actions in sneezing and coughing, and these again different from its actions in laughing and hiccup; after considering that our breathing is varied by heat and cold, by pleasure and pain, by every strong mental emotion, by the different states of health and disease, by different attitudes and different exertions,—we can hardly suppose that an animal under the influence of horror; placed in a forced and unnatural attitude; its viscera exposed to the stimulus of air; its blood flowing out; many of its muscles divided by the knife; and its nervous system driven to violent desultory action from excruciating pain, would exhibit the phenomena of ordinary respiration. In that situation its muscles must produce many effects, not only of violent but irregular action; and not only the muscles usually employed in performing the function, but also the muscles that occasionally are required to act as auxiliaries. If different anatomists, after seeing different species of animals or different individuals of the same species respiring under different experi-

ments of torture, were each to conclude that the phenomena produced in these cases were analogous to those of ordinary respiration, their differences of opinion as to motions of ordinary respiration would be immense!'

What Dr. Barclay here says about the fallacies inseparable from experiments on respiration will apply with greater force to other departments of physiology which have been investigated in a similar manner.

It would be easy to multiply testimonies, but there is space only to add the statements of one or two experimenters who have themselves admitted the uncertain and fallacious nature of their method of research. M. Legallois remarks in one place of his "Experiments on the influence of the nervous system on the circulation:" "J'eus presque autant de résultats différens que d' expériences; et après bien des efforts inutiles pour porter la lumière dans cette ténébreuse question, je pris la partie de l'abandonner non sans regret d'y avoir sacrifié un grand nombre d' animaux, et perdu beaucoup de temps."

The experience of M. Colin, a zealous advocate and extensive practiser of vivisection, is worthy of being noted. "Certain experiments," he says, "are complex in their nature when they are applied to important functions, the perturbations of which react on nearly the whole animal economy. Apply your instrument to the brain or the heart, and immediately you have general and serious disturbances of the system which it is necessary to disengage from those which belong to the direct and local result of the experiment." And again, with regard to the uncertainty of the results obtained, M. Colin says: "Often the same experiment repeated twenty times gives twenty different results, even when the animals are placed apparently in the same conditions. It may even happen that the same experiment gives contradictory results." M. Colin, after making this admission, speaks of the necessity for multiplying experiments: "It is necessary to recommence in order to learn."

The fairer and more philosophical conclusion would be, with M. Legallois, to desist from a mode of investigation which experience has shown to be unsatisfactory, and by the very nature of it, and of necessity, fallacious.

Sir Charles Bell said that "Vivisection has done more to perpetuate error than to enforce the just views taken from anatomy and the natural sciences." He said this chiefly in regard to the facts and principles of physiology. But the accusation holds good also as to the practice of the healing art, whether in medicine or surgery. Notable illustration of this has been given by Mr. G. Macilwain, F.R.C.S., in a recent treatise on vivisection, being chiefly short comments on portions of the evidence given before the Royal Commission. He proposed to prove to the Commission that experiments on living animals were not only useless and hindering more philosophical modes of research, but that they have been misleading, and so productive of great practical mischief in the practice of surgery. He was not allowed to do this, being courteously reminded that he was not before a medical committee. But he has since published what he intended to say, and his statements are valuable testimonies for those who seek to know the truth on the subject. Mr. Macilwain has been very long known as an eminent and successful surgeon, and in his lectures and books he has shown himself to possess much of the shrewd insight and independent thought of his great master, Mr. Abernethy.

The two illustrations of the misleading and mischievous influence of experiments on living animals are from the practice and the writings of Sir Astley Cooper and Mr. Travers, both men most popular in their day, and whose names have still great authority in the profession. The points selected by Mr. Macilwain seem to him good illustrations of the faults which are inseparable from vivisectional inquiries.

Sir Astley Cooper thought that when the neck of the thigh bone was fractured within the capsule enclosing the hip joint, repair by bony union was impracticable, and that union if effected could only be by ligament. That this mode of union was frequent after fracture of the neck of the femur he knew, but ligamentous union also is the mode of repair in other parts. Nay more, it was known that sometimes surgeons, after a while, purposely allowed some degree of motion in fractured bones, where they feared that the secretion of bone might be in inconvenient excess, and where ligamentous union took place. Besides, he knew that this fracture took place most commonly in persons advanced in life, when *unusual* care is necessary as regards the utmost quiet of the limb, so that no disturbance should occur in parts which it was essential to keep in apposition, and that various circumstances rendered this, in many cases, a matter of no small difficulty. Now, all this might be said to apply, more or less, to fractures in general, but it seems to have been lost sight of or unappreciated by Sir Astley. He had got the one idea of deficient reparative power, and seems to have referred failure to nothing else. Well, to prove this, as he thought, he made some experiments on animals; and here is another feature common in vivisection. A supposition is started, contrary to or irreconcileable with many known facts, or to some obvious analogy, and then experiments are made to see if it is true. So that, in a vast number of cases, a man commences his experiments, as Sir Astley did, with the disadvantage of a foregone conclusion. He accordingly experimented on dogs; and finding that the fractures he made in the thighs of the dogs only united by ligament, he regarded that as a confirmation of his doctrine. "Now," says Mr. Macilwain, "I will venture to affirm that not one of the circumstances necessary to the proper repair of the factured neck of the thigh-bone in the human subject could be accomplished in the dog, and especially that chief of all, the continually undisturbed condition of the injured parts.

But many surgeons, both here and on the Continent, took another view of the subject, and maintained that, if the parts were kept perfectly still, and so retained for the requisite time, the fractured neck of the thigh-bone would do just as well as others. Amongst these were Mr. Abernethy and Baron Larrey. Cases were successful, but were for a time met by the allegation that the fractures were outside or partially outside the capsule of the joint. As this could not be proved or disproved but by dissection, years passed during which the subject was matter of opinion. At length two or three cases occurred where opportunity was given to examine the joint after death, and the bony union of the fracture was fully established. But much evil had been done. Sir Astley was surgeon to one of the largest hospitals, and a leading teacher of surgery. Concluding that bony union could not be obtained in such cases, he recommended and adopted a practice which rendered it impossible. When the patient had been in bed a fortnight or so, and the inflammation consequent on the injury had subsided, he was made to rise and use a crutch, which, as rendering bony union impossible, necessarily involved lameness for life. The lamentable result of this practice of Sir Astley, though not warranted by a careful view of all the practical facts, but which, he concluded, his experiments on dogs seemed to confirm, can only be estimated by considering the number of cases submitted to his care, besides those of his pupils, who would probably, for a time at least, adopt the practice of their distinguished teacher.

Mr. Travers performed experiments on living dogs, causing a variety of injuries to the intestines, with a view to ascertain their powers of repair under these injuries. His inquiries seem specially to have been directed to the treatment of strangulated hernia. In writing of this disease, Mr. Travers says, that the great danger of the operation in strangulated hernia is from peritonitis. That is true;

but now, hear the remedy he proposes. "The great means to combat this is by purgatives. If there is no peritonitis," he says, "we give purgatives to prevent it; and if there is peritonitis, we give purgatives to cure it." It must be admitted that this use of purgatives has been common in the profession; but Mr. Macilwain thinks the treatment recommended by Mr. Travers worthy of special mention, because it shows how much he erred, if not in consequence of, certainly notwithstanding his experiments on animals. The probable explanation of the accession of peritonitis is, that where mucous and serous membranes are associated in the same organ, and the irritation of the mucous surface is accompanied by some obstacle which hinders the proper relief of the mucous, the irritation, or its effects, will be transferred to the serous membrane. To treat such a state by purgatives is an evident mistake, and sufficiently accounts for the great mortality under the treatment. Mr. Macilwain adopted successfully other measures to check inflammation, and he states that his predecessor, as surgeon to the London Truss Society—the elder Taunton—never gave purgatives, and had operated upwards of fifty times with only one or two failures. Whether Mr. Travers' treatment proceeded from what he did in his operations, or from what he neglected to do, it still illustrates the misleading character of vivisection, which failed to give useful guidance, when specially questioned by men so able and distinguished as Sir Astley Cooper and Mr. Travers.

The improvement in the mode of the ligature of arteries, introduced early in this century by Mr. Jones, has been ascribed to experiments on animals. These experiments may have confirmed his views, and satisfied others who saw them, but they were made in support of observation in the human body, which a few trials on small vessels, in the operating theatre, would have established far more speedily and surely. Yet this was presented by one witness to the Royal Commission as proving the necessity for experiments.

Nor were the arguments bearing on improvements in medical practice more conclusive. Take the experimental researches of Dr. Lauder Brunton in attempting to discover the pathology of cholera. His own account of the investigation, as given in his examination before the Commission, is as follows: "It was discovered by Moreau that by performing a certain operation upon the intestine you could get a discharge into the intestine. This discharge was discovered by Kühne to be exactly the same as was found in the intestine after cholera; so I thought, if we can find out the exact part of the nervous system that is concerned in causing this discharge, we shall probably be able to find out the part of the nervous system concerned in cholera; and having once found that out, we may be able to get a drug that will act upon it, and thus cure cholera." There were several series of experiments, in the first of which ninety cats were used. In using the knife chloroform was given, but the animals were allowed to live some time after they recovered. Dr. Brunton told the Commissioners that "they suffered a certain amount of discomfort, and *possibly*, pain, indeed *probably*, pain, though I do not think very great pain; I think probably not much more pain than a man would suffer who had perhaps a bad attack of diarrhœa." It was also said that they were killed in four or five hours.* It is not necessary to give the details of these experiments, nor do I wish to speak of them here as cruel, as the motive of the operator may have been humane. But what are we to think of the logic that led to the inquiry, and the wisdom in which it was carried out? Moreau found he could produce a discharge into the intestine; Kühne said this discharge was the same as that found after cholera; therefore, if we can find the part of the nervous system concerned

* This may have been the case in some of the experiments, but in the Bartholomew Hospital Reports of Dr. Legg, there are records of other experiments where the animals lingered for weeks.

in causing this discharge, we may find some drug that will act upon it, and thus cure cholera! The prospects of a cholera specific from vivisection are not very bright. Let us hope that not many hundreds of cats or dogs may be killed, in slow torture, before the unwisdom of the inquiry is recognised. The ninety cats of "the first series" of experiments might have sufficed.

If vivisection were really the luminous and fruitful method of research which its advocates represent it to be, physiology must long ere now have been the most advanced of the sciences, and none of the mysteries of animal life would remain obscure. For the last fifty years on the Continent, many men of high rank in science, learned and gifted, with well appointed laboratories and an unlimited supply of subjects for experiment—encouraged and applauded by the profession, and with no check or restraint from law or public opinion—have zealously cultivated this field of inquiry. In recent years many physiologists and biologists in England and America have entered into rivalry with those of France, Germany, and Italy. The experiments during the past half century may be reckoned by tens of thousands, some say even hundreds of thousands. Surely we may expect to have obtained abundant fruits from all this expenditure of labour and skill, of time and of life! Surely we may ask, what are the results of this long and unfettered investigation?

Admitting, as most medical men do, the abstract *right* to perform experiments on living animals for the advancement of science, with a view to the improvement of the healing art, the utility and value of this method of research may fairly be discussed. If unquestioned and important results could be shown, the protests against vivisection from the medical profession would be few. But vivisection has been tried and found wanting. The whole history of this branch of physiological research—from the time of Herophilus, Erasistratus, and the Egyptian operators who had

living human bodies to experiment upon, down to our own day, when Professor Ferrier has to be content with the anthropoid progenitor of the human race, and Professor Rutherford with man's faithful dependent, the dog, as the subjects for examination—the whole history of vivisection, if it does not convince men of science of the entire uselessness of these modes of research, will at least force them to admit that they are of infinitely less service than it is now the custom to represent them. Take any one of the particular subjects that have most occupied the attention of experiments—the functions of the various parts of the encephalon, for example—and what a mass of vague and absurdly discordant results appear as the fruit of all their researches! After the myriads of experiments by Legallois and Wilson Philip, Amussat and Fleurens, Magendie and Bouillaud, and by multitudes of others down to our own day, it is surely fair to ask what results can be shown. What facts are there, universally or even generally, admitted that can be truly described as the fruits of vivisection? A few conclusions, indeed, are given by experimenters as having been placed by them beyond the reach of controversy; but these few, I maintain, could have been as surely arrived at by anatomical and pathological research.

It is matter of regret, at the same time, that most men are not satisfied by the inductions obtained by legitimate means, and require for their conviction the visible demonstrations which the vivisector offers. It was thus with the great discovery of Sir Charles Bell, whose experiments, he expressly states, were performed, not for his own conviction, but for the satisfaction of others. It is the same with many of the vaunted discoveries of vivisectors, who gain ready reception and loud praise for the announcement and demonstration of facts already established by clinical and pathological research.

No opponent of vivisection denies that by means of it many facts in physiology can be demonstrated, and many

phenomena of animal life illustrated. No one denies that, while it is a method of research liable to much fallacy, and often apt to mislead or even to lead to wrong conclusions, it is also capable, on some points, of giving speedy and clear demonstration of facts and phenomena. The *conclusiveness* of many experiments on living animals is not disputed. For example, M. Magendie demonstrated that cutting off the eyelids of a rabbit, and leaving bare the globe of the eye, brought on ophthalmia. M. M. Bouley and Colin starved a horse, made an open wound in the throat, and injected some grains of strychnine, and the poor animal died in "characteristic convulsions." M. Fleurens removed with a knife some layers of the brain of a bird; "it immediately manifested a loss of harmony in its movements, it staggered, and fell." M. Bouillaud, who anticipated Professor Ferrier in his researches on the functions of the brain, conducted numerous experiments by injuring or removing various portions of the cerebral substance in different animals. In one of these he made an opening in the forehead of a young dog, on each side, and forced a red hot iron into the anterior lobes of the brain. "Immediately afterwards the animal, after howling violently, lay down as if to sleep. On urging it, it walked or even ran, for a considerable space; but it did not know how to avoid obstacles placed in its way, and on encountering them groaned, or even howled violently. Deprived of the knowledge of external objects, it no longer made any movements either to avoid or approach them. Yet it could still perform such motions as are called instinctive; it withdrew its feet when they were pinched, and shook itself when water was poured upon it. It turned incessantly in its cage as if to get out, and became impatient of the restraint thus imposed." These instinctive actions continued for several days, but no improvement appeared in its intellectual power, and it was killed because its irrepressible cries disturbed the neighbourhood. The anterior part of the cerebrum of another dog was removed, and the

results watched for several weeks. The details are too revolting to give, but the chief result noted is that the instinctive actions were not greatly affected. It ate with voracity, and when flung into the river swam on shore and returned to the house. But it acted "like an uneducated dog, whose intellect is undeveloped. When menaced, it crouches as if to implore mercy, but does not in consequence obey. Its want of docility was remarkable; when called it did not come, but lay down and wagged its tail with an air of stupidity." Experiments very similar to those of M. Bouillaud have been conducted by Professor Ferrier at the Laboratory of the West Riding Lunatic Asylum, and elsewhere. The brain being exposed by sawing away portions of the skull, the results of injury to various parts, by knife, concentrated acids, and by electric shocks, were observed and noted. Chloroform was usually administered, but the reports of the experiments show that the animals were not continuously under its influence, and sometimes were only "partially narcotised." Here, as in the French experiments, we read of the animals exhibiting signs of pain, fear, and rage. "The animal exhibited signs of pain, screamed, and kicked out with its left hind leg, at the same time turning its head round and looking behind it in an astonished manner." "When the temporo-sphenoidal gyri were being exposed the animal bit angrily, and gnawed its own legs.* It did the same generally after irritation of the same parts." "The excitability of the brain was now well-nigh exhausted, and entirely disappeared four hours after the commencement of the experiment, during which time the exploration was kept up uninterruptedly." Or take one of Professor Rutherford's experiments upon "the biliary secretion of the

* Dr. J. Crichton Browne told the Royal Commissioners that these were "merely mechanical movements," and that the animals were unconscious of pain (3189). Dr. Ferrier said he was most careful to avoid causing pain (3228). I must refer back to what I have said about anæsthetics, pp. 17-20.

dog." "Nine grains of podophylline, triturated in a mortar with some bile as a solvent, were injected into the duodenum of a dog, opened for the purpose. A rapid increase in the bile-secretion ensued; but soon it diminished, and three hours after the injection it was lower than it had ever been. In this remarkable experiment, therefore, the diminution of bile-secretion after podophylline was more marked than its increase; indeed, the increase might have possibly been owing to the injected bile, and not to the podophylline. Towards the close of the experiment the pulse became weak, but not excessively so. *Autopsy:* The mucous membrane of stomach and whole length of small intestine were intensely red. The small intestine contained a large quantity of fluid. The large intestine contained a considerable quantity of liquid fæcal matter. There was, therefore, abundant evidence that excessive purgation was imminent." The conclusion was that this large dose of podophylline, with a biliary solvent, produced intense irritation of the intestine, with signs of purgative power, but with effect on the liver not corresponding to the other results. Other experiments showed that when the intestinal irritation is less the biliary secretion is larger. The practical use of the experiments we are not now considering, but in these, as in other operations of the vivisectors, certain physiological facts are clearly exhibited. The same remark may be made as to the vast majority of experiments given in the "Handbook of the Physiological Laboratory," and in other manuals of vivisection. Our contention is not that such experiments are *inconclusive* in many instances, but that they are *useless*, and therefore cruel and immoral. Of many of them we affirm that the facts ascertained have no bearing either on the general principle of physiology, nor on the practice of medicine. And of others, which seem to bear some relation to the advancement of knowledge or art, we affirm that the results could be and are attained by clinical and pathological observation. In the words of Celsus, which may be taken as

the motto for this essay, "Hæc cognoscere prudentem medicum non cædem sed sanitatem molientem; idque per misericordiam discere, quod alii dirâ crudelitate cognoverint." And again, "Ex iis quœ violentiâ qmæruntur, alia non possunt omnino cognosci, alia possunt etiam sine scelere."

We are now prepared for considering the question, Are experiments on living animals morally justifiable? The question cannot receive a direct and categorical reply, irrespective of motives and of results. Man's dominion over the lower animals is very large, and it is his, not only by superior knowledge and power, but by Divine appointment. The dominion is not absolute, but limited by the eternal obligations of justice and mercy. Man may use this delegated dominion for his own benefit, but he may not abuse it. The gentle and genial poet Cowper, has well expressed the extent and the limit of this dominion:—

> "The sum is this,—if man's convenience, health,
> Or safety interfere, his rights and claims
> Are paramount, and must extinguish theirs:
> Else they are all, the meanest things that are,
> As free to live, and to enjoy that life,
> As God was free to form them at the first,
> Who in His sovereign wisdom made them all."

For the food, the clothing, and other uses of man, many animals are sacrificed. None but Brahmins, on religious grounds, or some Vegetarians, on grounds partly dietetic and partly ethical, object to taking the life of the lower animals for such purposes. Even when life is not taken, animals may be put to pain and may suffer injury, as in castration, to fit them for the useful service of man. On the same principle, it can fairly be argued that man has right to use animals for researches that may lead to the restoration or the preservation of human health. But, before we admit this, we must be satisfied that the results of these researches are such as justify the resort to them,

and also that these results can be obtained no other way. This is what we have investigated in the previous part of our essay, and have concluded that, on this plea, they are not justifiable.

There is another way of looking at the question of vivisection, as tending to human benefit. If it is right to perform experiments on living bodies for advancement of the healing art, why not perform them on human bodies? It has been done in past times, and may be proposed again. If condemned malefactors were operated upon, it would only be anticipating, by a brief period, their hour of death. Or the experiments might be made on the insane and imbecile, or persons defective in intellectual or moral faculties, but with animal life in natural vigour. These subjects would be free from the objections arising out of the different structure, constitution, and functions of the lower animals, though still liable to certain fallacies inseparable from the very method of research. Vivisectors make light of these alleged fallacies, and think their experiments full of light and fruit. Fair argument might be used for experiments on living men, with or without anæsthetics, as the inquiry might demand. It might be argued that it is expedient or right that one or a few should suffer for the benefit of the human family. And if the argument, '*in majus bonum*,' were strengthened by reference to *corpora vilia*, then of malefactors doomed to die, and of imbeciles, this could be truly said.

Vivisectors would hardly venture, at least not yet—at least in England—to propose returning to the practice of experiments on living human bodies. Public opinion, and medical opinion, would revolt from the proposal, if biologists and physiologists should propose it.

And why? Not because the arguments for such experiments are weak, but because the objections of the moral sense are strong. Except for self-defence or self-preservation, the moral sense recoils from the infliction of pain and

injury, even when a lofty motive may be urged. Why has trial by torture been banished from the jurisprudence of every civilised nation? The object of the rack, and the the thumbscrew, and of all the infernal apparatus in use in our courts of law at no very remote period, was not to cause pain, far less to give any satisfaction or pleasure. The discovery of truth was the object in this method of interrogation; and with this end in view, the use of torture was justified, and directed by rulers and judges, in other respects humane as well as just. In the still more horrible tortures of the Inquisition, the object was not avowedly that of vindictive punishment; nor need we assume that even the lowest executioners and officers of that dark tribunal took pleasure in the agonies of their heretic victims. The professed aim was higher even than in the processes of ordinary torture in courts of law. The advancement of Divine truth and of sacred science, or theology, was the alleged design of the Inquisition, while the spiritual welfare and eternal salvation of men might be also attained, through subjecting them to short though sharp affliction. Yet examination by torture is advocated by no one, because the infliction of pain, even for the advancement of truth, is not justifiable. And does not this apply with equal force to experiments on the lower animals? This is, indeed, interrogating nature by torture! You might operate on human subjects with no higher intelligence, and of no higher moral condition, and certainly with no more sensitive frame, than the poor brutes that are carried to the vivisector's laboratory. It is the infliction of pain and injury that cannot be justified, whether the victim be an imbecile human idiot, or a docile intelligent dog.

Professor Newman, in a published letter, has said, "Evidently the reason why it is wicked to torture a man is not because he has an immortal soul, but because he has a highly sensitive body, and so has every vertebrate animal,

especially the warm-blooded. If we have no moral right to torture a man, neither have we a moral right to torture a dog." And again, "We have to add to our morals a new chapter on the Rights of Animals. Men who teach to trample them down are teachers of hard-heartedness, and are real enemies of mankind, while they undertake to promote human welfare."

There is a remarkable passage in the works of Jeremy Bentham, applying the principle of natural law to the rights of animals. It is quoted by Sir Arthur Helps in his "Talks about Animals and their Masters." "The day may come when the rest of the animal creation may acquire those rights which never could have been withheld from them but by the hand of tyranny. It may come one day to be recognised that the number of legs, the villosity of the skin, or the termination of the *os sacrum*, are reasons insufficient for abandoning a sensitive being to the caprice of a tormentor. What else is it that should trace the inseparable line? Is it the faculty of reason, or perhaps the faculty of discourse? But a full-grown horse or dog is beyond comparison a more rational as well as a more conversable animal than an infant of a day, or a week, or even a month old. But suppose the case were otherwise, what could it avail? The question is not, 'Can they reason?' nor 'Can they speak?' but 'Can they suffer?'"

If Justice requires that the rights of animals should be respected, and questions of wrong-doing not be confined to man's treatment of his fellow-men, much more does Mercy refuse to recognise the arbitrary limit of our own species. "There is implanted by Nature," says Lord Bacon, "in the heart of man, a noble and excellent affection of mercy, extending even to the brute animals, which, by the Divine appointment, are subjected to his dominion."

Dr. Chalmers, in his eloquent sermon, says of humanity to the lower animals:—"It is a virtue which oversteps, as it were, the limits of a species, and which prompts a descend-

ing movement on our part, of righteousness and mercy towards those who have an inferior place to ourselves in the scale of creation. It is not the circulation of benevolence within the limits of one species. It is the transmission of it from one species to another. The first is the charity of a world. The second is the charity of a universe. Had there been no such charity, no descending current of love and of compassion from species to species, what, I ask, would have become of ourselves? . . . The distance upward between us and that mysterious Being, who let Himself down from Heaven's high concave upon our lowly platform, surpasses by infinity the distance downward between us and everything that breathes. And He bowed Himself thus far for the purpose of an example, as well as for the purpose of an expiation, that every Christian might extend his compassionate regards over the whole of sentient and suffering nature." By Dr. Chalmers the duty of mercy to animals was thus lifted to the highest level of Christian ethics. In the same spirit are the words of a distinguished man of science and Christian philanthropist, Dr. George Wilson:— "There is an example as well as a lesson for us in the Saviour's compassion for men. Inasmuch as we partake with the lower animals of bodies exquisitely sensitive to pain, and often agonised by it, we should be slow to torture creatures who, though not sharers of our joys, or participators in our mental agonies, can equal us in bodily suffering. We stand, by Divine appointment, between God and His irresponsible subjects, and are as gods to them."

May we not say that vivisection is thus contrary alike to the justice which regards the rights of animals, and to the mercy which has sympathy with the helpless and the suffering? In the principle of the thing, man has no more right to perform painful or injurious experiments on animals than on human beings.

I have said that man's dominion over all living creatures is not absolute, but limited by the eternal obligations of

justice and mercy. It is also to be regarded not merely as a right but as a trust. On this point I quote some sentences from a remarkable speech by the great Lord Erskine, when he was trying to induce the Government of his day to legislate for the protection of animals from cruelty:—"That the dominion of man over the lower world is a moral trust, is a proposition which no man living can deny, without denying the whole foundation of our duties. If, in the examination of the qualities, powers, and instincts of animals, we could discover nothing else but their admirable and wonderful construction for man's assistance; if we found no organs in the animals for their own gratification and happiness—no sensibility to pain or pleasure—no senses analogous, though inferior to our own—no grateful sense of kindness, nor suffering from neglect or injury; if we discovered, in short, nothing but mere animated matter, obviously and exclusively subservient to human purposes, it would be difficult to maintain that the dominion over them was a trust, in any other sense at least than to make the best use for ourselves of the property which Providence had given us. But it calls for no deep or extended skill in natural history to know that the very reverse of this is the case, and that God is the benevolent and impartial author of all that He has created. For every animal which comes in contact with man, and whose powers and qualities and instincts are obviously adapted to his use, Nature has taken care to provide, and as carefully and bountifully as for man himself, organs and feelings for its own enjoyment and happiness." "The animals are given for our use, but not for our abuse. Their freedom and enjoyments, when they cease to be consistent with our just dominion and enjoyments, can be no part of their natural rights; but whilst they are consistent, their rights, subservient as they are, ought to be as sacred as our own."

Having stated the ethical principles on which the opposition to vivisection is founded, and shown that the system

is not in harmony with the moral government of the world, there remains an important practical question as to the moral effects of this mode of research. Is not the tendency to harden the operator, and blunt his moral sense? And, if so, is not the system injurious, not only to those engaged in it, but to the tone and character of the medical profession, and to society at large?

In examining the question, in its moral and social bearings, it is of no avail to say that some vivisectionists are good and exemplary, and even tender-hearted men. This is true; and it may be also admitted that, in the performance of experiments, they themselves are subjected to much mental distress. Nothing but a high sense of duty, and an earnest desire to obtain useful results, could induce medical men of culture and ordinary feeling to engage in some of the researches, the mere descriptions of which cannot be read without pain and horror. Professor Rolleston, of Oxford, in giving his evidence before the Royal Commission, bore testimony, from personal intercourse and friendship, as to the amiable character of some experimenters. One of these was a joint-author of the Handbook for the physiological laboratory. A very terrible experiment was quoted in that book; and being asked how he accounted for any humane person inserting it as an illustration, Professor Rolleston said that Dr. Foster had never shown it (the experiment on recurrent sensibility), and never seen it himself. Asked: "But surely it is put here, in a Handbook, in a mode which would encourage the trying of that experiment?" The reply was: "Obviously; but I am speaking in vindication of the character of my friend, but not at all in vindication of the book." Asked: "Then I understood that your opinion about the book is, that it is a dangerous book to society, and that it has warranted, to some extent, the feeling of anxiety in the public which its publication has created?" "I am sorry," replied Professor Rolleston, "to have to say that I do think that is so."

Others have shown no reserve at all in defence of everything contained in it, and have exhibited a defiance of public opinion too plainly arising from callous indifference. That they should be supported by men of more gentle and refined nature only proves the more strikingly that the tendency of vivisection is to blunt the moral sense. It is a law in ethics, that the strength of any motive is increased or diminished, according to the habitual exercise of the mental emotion brought into play. Sympathy for distress and aversion to inflict pain may be naturally strong in the heart of a biologist or physician, but may be gradually overpowered and suppressed by the habitual exercise of other motives, such as zeal for science or ambition of scientific fame. Every time these passions prevail an increased purchase is gained for their future influence, and the heart is hardened as they encroach on the rightful domain of sympathy and compassion for poor suffering animals. In other persons, the better feeling of possibly rendering good to men by improvements in medicine, represses the immediate emotion of pity; and even humane physicians advocate the most fearful proceedings of vivisection. Such is the natural process by which the feelings are blunted and the moral sense restrained from protesting against the cruelty of vivisection.

While thus explaining the personal blunting of feeling towards animals, in some who may be amiable and kind to their fellow-men, no reserve should be maintained in declaring the evil tendency of the system. To those who possess the large Blue Book, with the reports and evidence of the Royal Commission, or who have, in other ways, specially become acquainted with the history of vivisection, it would be needless to offer proofs on this matter. But a large proportion of the medical men of the day know little of what has passed in regard to the teaching of physiology in recent years. This is a new feature in English medical education. There were no physiological laboratories, not

even class demonstrations, in our student days, at Guy's or St. Bartholomew's; nor at the Universities of Edinburgh or London was the practice of vivisection recognised. The altered attitude of the medical press, and of the official representatives of the profession, already show signs of deterioration of moral and social tone, and there is need for plainly showing the influences now at work, and leavening the character of the rising race of medical practitioners.

Dr. George Hoggan published in *Fraser's Magazine*, for April, 1875, a statement of what he had witnessed as assistant in the laboratory of one of the most eminent physiologists of France. The name is courteously withheld, but it is very well understood to what place the reference is made. "In that laboratory," says Dr. Hoggan, "we sacrificed daily from one to three dogs, besides rabbits and other animals, and after four months experience, I am of opinion that not one of these experiments was justified or necessary. The idea of the good of humanity was simply out of the question, and would have been laughed at, the great aim being to keep up with, or get ahead of, one's contemporaries in science, even at the price of an incalculable amount of torture needlessly and iniquitously inflicted on the poor animals.

"During three campaigns I have witnessed many harsh sights, but I think the saddest sight I ever witnessed was when the dogs were brought up from the cellar to the laboratory for sacrifice. Instead of appearing pleased with the change from darkness to light, they seemed seized with horror as soon as they smelt the air of the place, divining apparently their approaching fate. They would make friendly advances to each of the three or four persons present, and as far as eyes, ears, and tail could make a mute appeal for mercy eloquent, they tried it in vain. Even when roughly grasped and thrown down on the torture trough a low complaining whine at such treatment would be all the protest made, and they would continue to lick the hand

which bound them till their mouths were fixed in the gag, and they could only flap their tail in the trough as their last means of exciting compassion. Often when convulsed by the pain of their torture this would be renewed, and they would be soothed instantly on receiving a few gentle pats. It was all the aid or comfort I could give them, and I gave it often. They seemed to take it as an earnest of fellow-feeling, that would cause their torture to come to an end—an end only brought by death.

"Were the feelings of experimental physiologists not blunted, they could not long continue the practice of vivisection. They are always ready to repudiate any implied want of tender feeling, but I must say that they seldom show much pity; on the contrary, in practice they frequently show the reverse. Hundreds of times I have seen when an animal writhed with pain, and thereby deranged the tissues, during a delicate dissection, instead of being soothed it would receive a slap and an angry order to be quiet and to behave itself. At other times, when an animal had endured great pain for hours without struggling or giving more than an occasional low whine, instead of letting the poor mangled wretch loose to crawl painfully about the place in reserve for another day's torture, it would receive pity so far that it would be said to have behaved well enough to merit death; and, as a reward, would be killed at once by breaking up the medulla with a needle, or 'pithing,' as this operation is called. I have heard the Professor say, when one side of an animal had been so mangled, and the tissues so obscured by clotted blood that it was difficult to find the part searched for, 'Why don't you begin on the other side?' or 'Why don't you take another dog?' 'What is the use of being so economical?'

"One of the most revolting features in the laboratory was the custom of giving an animal on which the professor had completed his experiment, and which had still some life left, to the assistants, to practice the finding of arteries, nerves,

&c., in the living animal, or for performing what are called fundamental experiments upon it—in other words, repeating those which are recommended in the laboratory handbooks."

Such was Dr. Hoggan's experience in the laboratory of one who was in the first rank in Paris as a physiologist. His words are worth repeating. "I am of opinion that not one of those experiments on animals was justified or necessary." The wonder is how he could have assisted at such scenes of torture, as he calls them, for so long a period. It is well that he has now made so clear a statement, and generous a confession. His evidence may serve as a warning as to what is possible in England, if this system of research spreads among us. Another English surgeon, visiting a French laboratory, describes the conduct of the students, in mimicking the cries and moans of the tortured animals in derision, as so revolting that he quitted the place in disgust. I myself witnessed, long ago, this "tiger-monkey" spirit in Magendie's class-room. Along with the late Edward Forbes, and two or three other students from Edinburgh, I tried to learn something from Magendie, but we were driven from the place in disgust, shocked not so much by the coarse cruelty of the Professor as by the repulsive heartlessness of the spectators. English students were not in those days accustomed to such scenes of horror. The foreign teachers know the greater sensitiveness of our countrymen, although the honourable distinction seems to be passing away. An English student having quitted a well-known German laboratory, unable to bear its horrors, the professor said that "he never found Englishmen who would stop with him, and he supposed (with a sneer) that they thought God would make them suffer the same as the animals."

The experience of the last few years sadly proves how soon and how effectually the tone, which has distinguished English from Continental schools, has been lowered. Ten

or twelve years ago Mr. Fleming, author of the first prize essay published by the Society for Prevention of Cruelty to Animals, after describing the fearful cruelties daily witnessed at the College of Alfort, the chief veterinary training school in France, could say that, "To the honour of the veterinary schools of England, vivisection has never been allowed in them;" and Mr. Fleming, with just pride, adds, "No one will deny that they are as well qualified to undertake the management of difficult operations as the vivisectionists." The details of the practices at Alfort, and also at Lyons, as given by Mr. Fleming, form a most ghastly record. The scandal caused by these atrocities led to an appeal being made to the late Emperor of the French, who referred the matter to a Scientific Commission. The practices are, however, continued to the present day, and, we grieve to say, have been introduced into this country. Mr. James Mills has put on record a fearful account of cruelties which he witnessed, and in which he took part when attending the Edinburgh Veterinary College, but of his share in which he is now heartily ashamed. Both veterinary and medical students joined in the experiments which Mr. Mills describes. He says, "There was no other motive than idle curiosity, and heedless, reckless love of experimentation. To observe the heart's action a cat was fastened down on its back. An incision through the skin of the animal's chest, extending from the neck to the belly. The skin was then laid back by hooks, to enable the operator to cut through the cartilage of the sternum, and to draw his knife across the ribs for the purpose of nicking them. The ribs were then snapped, and the fractured parts turned back and secured by hooks. No anæsthetic was used. On another occasion a horse was bought for the purpose of dissection. During a whole week this animal was subjected to various operations, such as tenotomy, neurotomy, &c., again without anæsthetics. In other cases the animals received "brutal usage." Mr. Mills exonerates

the professors from participation in the experiments, most of which were performed in the students' lodgings; but the Principal must have known of the horse being experimented on within the walls of the College. It is not surprising that Dr. Haughton, of Dublin, in his evidence before the Commission, said: "I would shrink with horror from accustoming large classes of young men to the sight of animals under vivisection. I believe that many of them would become cruel and hardened, and would go away and repeat those experiments recklessly. Science would gain nothing, and the *world would have let loose upon it a set of devils.*"

Dr. Acland, of Oxford, said, in his evidence, that many persons are now engaged in the pursuit of vivisection in this country, not for a humane purpose, but for acquiring abstract knowledge. This desire of mere discovery has a dangerous and mischievous tendency. "So many persons have got to deal with those wonderful and beautiful organisms just as they deal with physical bodies that have no feeling and consciousness." Dr. Acland said this could not be done without being so hurtful to the moral sense of England that it would not be endured if carried to the same extent as abroad. Surely an effort must be made to prevent our English schools of medicine being degraded to the Continental level.

Much has been said about the evidence of Dr. Klein, Director of the Brown Institution, and Lecturer on Histology at the Medical School of St. Bartholomew's Hospital. He certainly made some candid and strange admissions as to the cruelties alleged to have taken place in his researches. He said that a physiologist could not be expected to devote time and thought to inquiring what the animal feels while he is doing the experiment. He "uses anæsthetics only for convenience sake, in dogs and cats, and for no other animals as a general rule." Dr. Klein must not be too severely judged. His training has been different from that of most

Englishmen; and he never knew in Vienna, where he formerly practised, any of the hostility to vivisection which is common in this country on the part of the general public, though not of physiologists.

But Dr. Klein's statements lead us to view with dark foreboding the avowed opinions of some of our leading professors and public teachers, as when Dr. Burdon-Sanderson says he "wishes to see the type of education here more like the type of education in Germany." Dr. Gamgee, of Manchester, also praises highly the proceedings of Dr. Ludwig, of Leipsic, who has been the teacher of nearly all the physiologists of Europe, and has indoctrinated nearly the whole of them in the methods of physiological inquiry. These expressions of opinion, from prominent and representative men, and still more, the reported proceedings of the General Medical Council and of the British Medical Association, in reference to legislation on the subject, give rise to sad forebodings for the future. The new generations of medical men, trained under such influences, although few of them may have been personally engaged in experiments, must become degraded in moral and social tone, and the whole status of the profession will thereby be affected.

Foolish things may have been said, and extreme views held by those who advocate the total abolition, or suppression by law, of experiments on living animals. Even those who most wish this can scarcely hope to see their wish realised. But I do not despair to see such a change in the general opinion of the profession regarding such experiments as will render them of rare and exceptional occurrence. Apart from any ignorant clamour there is a strong public feeling as to the cruelties of vivisection. Sir Arthur Helps gave expression to the feeling prevalent among men of culture in all professions, when he said that "any man known to have practised needless cruelties on animals should be placed under a social ban." It is very certain that the status of the profession may be lowered by beng associated

in the public mind with vivisection. There are already signs of this, and many medical men would rejoice to see their profession delivered from the opprobrium that has come upon it in consequence of this practice. This can be done only by showing that sound science is on the side of humanity on this question. So far from vivisection having aided in the advancement of the healing art, many testimonies confirm the saying of Sir Charles Bell, that "it has done more to perpetuate error than to add to sound knowledge." At all events the advantages of such experiments have been vastly overrated, and their disadvantages not duly considered. The question is not whether any results are obtained from this source, but whether they are worth the price paid for them. That knowledge is dear which is purchased at the expense of humanity. These experiments involve much suffering and wrong, afford very meagre and doubtful results for practical use, and withdraw attention from sounder methods of research. They are neither scientifically valuable, nor morally justifiable.

A HANDBOOK

FOR INVESTIGATORS INTO THE SCIENTIFIC AND MORAL CLAIMS AND DEFENCES OF THE SYSTEM OF EXPERIMENTING ON LIVE ANIMALS FOR THE PROMOTION OF SCIENCE,

COMMONLY CALLED

VIVISECTION:

OR,

PAINFUL EXPERIMENTS ON LIVING ANIMALS

SCIENTIFICALLY AND ETHICALLY CONSIDERED.

BY THE

REV. BREWIN GRANT, B.A.,

Vicar, St. Paul's, Bethnal Green, London, E.

MOTTO:

"Our hands without doubt are EMPTY to-day, but our mouths full, PERHAPS, of *legitimate* promises for the future."—(Lessons to the College of France, by Claude Bernard, page 43, Paris, 1877.)

Royal Commission Question (1955):—"Getting no result is getting a very great result. You have decided at all events that a *particular agent* does nothing? (Mr. Huxley)."—[*True* as applied to experimenting physiology.]

PREFACE.

A CAREFULLY compiled production, that would fulfil the promise of the above title is one great *desideratum* of this age.

Such a Handbook should present a short, clear, comprehensive view of the entire question, in a manner that would commend itself to the respectful consideration of the advocates and practisers of that system, re-stating and fairly meeting the arguments adduced in its defence.

It should be suitable, in scientific accuracy, for practising Physiologists and the Medical profession, and yet so untechnical in explanatory statement and style of reasoning as to be intelligible to inquiring, educated people; useful to speakers and lecturers on social and moral questions; and should contain or constitute a quiet, forcible appeal to the enlightened conscience of a civilised Christian community.

Its force should consist of invincible argument, which, without declamatory exclamations, should not only win the assent of an intelligent public, as a matter of plain right, and of the scientific enquirer, as to the most

legitimate and advantageous methods of promoting real science, but should also force the practising and "professed Physiologists" either to attempt some defence or exculpation more satisfactory to the understanding and conscience of the public, than that on which their present "*license*" is founded; or to retire from an indefensible practice, and from institutions, which, depending on the public for support, should not be the secret asylums in which to carry on operations that are condemned on logical, philosophical, and scientific grounds, as well as by "the general sentiment of humanity," and "the moral sense of the community at large."

In case the Handbook for Investigators into the system of experiments on live animals, for the alleged promotion of science, should fail to produce either of the above effects on "people who" "belong to our craft," as Dr. Burdon Sanderson defined "professed Physiologists" to the Royal Commissioners, (Question 2265)—or persons "accomplished in physiological matters," as Mr. Huxley described them, (3324), then it should at least give a clear, intelligent groundwork for public action, so that the will of the nation shall be expressed and not suppressed or evaded, by the decisions of the Legislature, but be carried out, and that we may no longer, as a nation in our corporate capacity, be open to the charge of condoning, by secret license, that which the conscience of the individuals constituting the community, laments and condemns.

It was acknowledged by the Commissioners appointed to inquire into this subject, in 1875–6, that "a feeling of

suspicion and even of abhorrence had grown up *amongst a large and very estimable portion* of the public," of which they said, somewhat ambiguously, "it is much to be regretted that it should have been *permitted* to grow up," as if there were some legitimate authority that should have prevented the formation of that public opinion.

It was this "feeling," not among fanatical and violent partisans, but "among a large and very estimable portion of the public," which afterwards was given as an excuse, by the Home Secretary, for *withholding the names* of the licensed experimenters from the public.

Such a course of defence cannot be relied upon as permanent, in a free country, and therefore some *better defence than secrecy* as to operators and operations, some line of argument that shall justify this system to the intelligence and "moral sense" of the community, must be adduced; or the protection to Physiologists by license will be as precarious as the protection to animals by anæsthetics.

The arguments of this Handbook are intended to call out such improved defence, or to force a surrender of the system.

Amongst the few cheerful witticisms which relieve the evidence given before the Royal Commissioners, there is one by Mr. G. H. Lewes:—"It seems to me that the vivisection, of which we are now speaking, is very much like vivisection in another department, that of Literature, that is to say, *criticism, which is also vivisection*," (6334). Of course he means when performed on *live* authors; for he

says, "There is a great deal of torture inflicted upon authors by critics, which lasts for a considerable time in sensitive minds. 6335. Sir B. Karslake—*And without anæsthetics?* Without anæsthetics. 6336. Mr. Erichsen—And by incompetent persons? Not only incompetent persons, but by persons who, even when they *are* competent, are often reckless." Still we are assured, in this last answer, "it is quite true that for the benefit of literature, and consequently of society, *criticism is a necessity.*"

It will therefore be permitted to perform *this kind* of "vivisection," in the way of logical "investigation" into the line of argument and defence put forth in favour of experimenting on animals.

The triumphs of the method are well summed up in Claude Bernard's latest farewell declaration:—

"*Our* HANDS are EMPTY," "but our mouths are FULL,"

which should be *the Epitaph* of experimenting physiology, recording—that it promised much and did nothing.

CHAPTER I.

THE TWO PROFESSIONS — MEDICAL AND PHYSIOLOGICAL — SHOWING THEIR RESPECTIVE PROVINCES AND RELATION TO EACH OTHER. THE CLAIM OF THE PROFESSED PHYSIOLOGISTS IS TO LAY DOWN—BY THEIR EXPERIMENTS ON LIVE ANIMALS—A "RATIONAL" AND "SCIENTIFIC BASIS" FOR MEDICAL PRACTICE; GIVING "CERTAINTY," "PRECISION," AND EFFICACY TO THE TREATMENT OF ORDINARY OR NATURAL HUMAN MALADIES, BY THE KNOWLEDGE OBTAINED FROM INVESTIGATION INTO IMITATION MALADIES AND DISEASES, ARTIFICIALLY PRODUCED IN ANIMALS.

THE special defence of the experimenting method is its aid to the medical profession in treating, preventing, and assuaging human suffering.

It leaves to the medical profession all practical observations on the actually occurring conditions of humanity; while, under the name of "physiology," it explores as its own special "field" such morbid and abnormal conditions as by drugs and a variety of cutting operations can be produced in animals by way of experiment.

This is its line of work, and the declared object by which it is justified is—the good of humanity, in the practical advantages secured by it to medical science.

Its value, as confessed by its promoters, is to be judged by the aid it gives in the way of beneficial knowledge not attainable from any other sources. This value is not to be measured by the estimate which experimenters give of their work, but by what they actually accomplish.

The question as to the cost, in the way of suffering inflicted on animals, leaves the region of science and enters into that of Ethics—how far we are justified in seeking selfish advantages at the expense of sensitive creatures. The surgical operator inflicts pain on his voluntary patient, for that patient's own ultimate relief; the working physiologist carries on, in a systematic, wholesale way, a series of painful inflictions on animals, not for *their* relief, but for the relief of man, by the aid of such light, as,—obtained from the laboratory of animal suffering, is handed down for the guidance of the medical practitioner, who is assumed to be dependent on such guidance, for any "certainty" even as to what he already "suspects," and thinks he knows, and actually practises; as well as in reference to any "advance" towards what has not yet been "suspected."

For this is the phraseology by which medical knowledge, gained by long patient experience, practice, and observation, is depreciated; all such is said to be what has been "*suspected,*" but when some real, if accidental accordance with what has been long, if not always, "suspected" and acted upon, in relation to man, is found or alleged in some experiment on an animal, *then* it is "converted" into "certainty" by the experimenter; and the medical man is henceforth permitted to "know" what he was before only allowed to "suspect."

This is what they call "giving preciseness and certainty to our knowledge," and it seems slightly affected and pedantical, though it is scarcely beneficial.

There are two lines of defence connecting "professed physiology" with medicine as its scientific guide and benefactor; *First, Discoveries;* and *Secondly, the Benefits* to Surgery and Medicine, which are alleged to have followed from these assumed discoveries by experiment.

The real "investigator" will carefully notice the subsequent collapse of the defence, when, from *beneficial* knowledge, the advocate of the value of experimenting, turns to "knowledge" without benefits, and claims "the advance of

science," or some recondite speculation, as itself as good as a benefit, and indeed as a great deal better.

This serves as a cover for the want of that real benefit to humanity on which the theory is founded, and from which this allegation of "the advance of knowledge" is a skilful retreat or "strategic movement."

When "Benefits" to medical practice, on which physiologists base their justification, are not at hand in any case of alleged discovery, the "discovery" itself is made into a "Benefit;" and that same physiology, which exists at first only to advance the medical art, is next made into a sufficient end of itself; it is enough to advance physiological science, a conglomeration of theories, or Molecular Metaphysics, of more interest to the speculative fancy than of practical advantage to suffering humanity.

Thus Dr. Sharpey *ends* a paper addressed to the Royal Commissioners on Experimenting (Question 394), by pointing out some alleged benefits, and then evacuates the main position—that physiology is good, because its experiments promote medical science; and retires on the position that the experiments are good because they promote physiology. He says:—"As I said before, I should *lay less stress upon the direct application* of the *results* of vivisection *in the art of healing, than* upon the value of these experiments in the *promotion of physiology* as a science." (Question 394 at the end; p. 20; col. b.)

What "physiology" *is* as "a science," in the sense appropriated to it by the "professed physiologists" of the experimenting school, we may yet examine; it is enough to here point out its assumed relation to the "healing art;" it exists first to *aid that;* and then it exists mainly for "promotion of physiology as a science," if not for "the promotion of" experimenters, to lecture rooms and laboratories, which, for their legitimate uses, might be as well adorned by able medical men as by these specialists.

The Royal Commissioners in their Report thus describe

the "Physiologists"—as the experimenters call themselves pre-eminently:—"Those who are *devoted to the improvement of medicine, and* to the advancement of *science*" (xvii.).

This is a two-fold or amphibious description; so that, as occasion may require, *either* object may be pleaded when the *other* fails, according to the description in Hudibras:—

> "Creatures of amphibious nature
> Can either live in land or water."

Sometimes the "advancement of science" is the means of "the improvement of medicine" as its beneficial result; at other times, it becomes an end as a separate purpose; and finally, in case of emergency, it becomes in itself a sufficient if not the sole purpose.

But at this stage, for our present investigation, it is "for the improvement of medicine;" that is, people are at work in this particular way on animals to give certainty and scientific precision to the treatment of human patients by medical practitioners.

We are assured how necessary this is on the ground that medicine, as managed only by the Faculty, is "empirical," and founded on what Mr. Huxley calls "the rule of thumb" (1770), instead of being, as the experimenters aim to make it, "founded on a rational basis."

"I think," says Dr. Sharpey, "that vivisection is of value as promoting the science of Physiology; and that again, is *one* of the great *foundations of all rational* medicine." "IT PUTS A LAMP, so to speak, *into the hand of the Physician*, when he is studying disease." (394; p. 20; col. a, at foot.) Without the aid of *that* "Physiology," which is obtained—like the knowledge of the old Latin Aruspices—from inspections of the entrails of animals, the physician would be groping his way in the dark—"so to speak." Whether that "Lamp" is ever lighted, whether the professed physiologists have any "oil in their vessels" to keep it lighted, or whether

at best it gives only a smoky and dingy glare just to make "darkness visible," is the subject of our enquiry.

It is the light alleged to be given to medicine by this physiology which is to condone all the cruelty and suffering involved in its experiments. Dr. Handyside (5951) puts it thus:—"I say, in short, that *experiment is the basis of medicine.*" "I said that medical science is in its *infancy,*" and that fact "points to the necessity" of "vivisection;" "and I believe fully in vivisection, *otherwise* [that is, without it] medicine, which is an *inexact, empirical,* or tentative science, *not like* chemistry or mathematics, could not advance any further." "We are *getting* EXACT *knowledge,* and I believe *that* is very much dependent on *experiments* on the lower* animals, *done humanely and mercifully.*" What the last four words have to do with the subject we have no "exact knowledge." They are an irrelevant flourish.

Speaking of some experiments on horses and dogs, Dr. Handyside explains, in effect, that he did them out of a "humane and merciful" regard for the pitiful state of the healing art:—"It was not willingly done;" "it was my duty to vivisect these animals," "with which I had *much* sympathy; and *quite justifiable,* seeing it is ONLY *in that* way that *an empirical* and inexact science, like *medicine, can be advanced*" (5994). Whether *experiments* are not just *literally* themselves "*empirical,*" and, whether the conclusions drawn from them, are not generally "inexact" and often contradictory, is well worth the consideration of "professed physiologists."

The proper professors of this "craft" are not medical men; and the same professors teach that medical men need not and should not be adepts in this "mystery," though it is said to be essential for their art. *It is a speciality.*

This brings us, from a consideration of the relation of

* The phrase "*lower* animals" occurs too frequently, and is scarcely complimentary to the higher "animal"—Man.

the two subjects, physiology and medicine, to consider separately the professors of these two lines of work; especially those professing "physiology," as a comparatively *new school*, introduced from abroad, to nurse medicine during its "infancy."

Foreign "physiology," as experimenting is called, was more fully introduced into England by the Handbook for Beginners, composed by Drs. Sanderson, Klein, Foster, and Brunton, mainly from accounts of Continental processes.

One of the four, Dr. Michael Foster, described to the Commissioners, and no doubt truly, as a gentleman of an exceptionally refined mind, calls himself "a home-bred physiologist," and declares that he is not "practically" acquainted with foreign experiments. That Handbook, which gave rise or intensity to the public feeling or "furore" respecting experiments, and was the text-book of the English-Foreign School, was in some respects extremely unfortunate: it *omitted Anæsthetics* where they might be applied; it gave, as for performance, cruel experiments; it was described in its title as "for beginners," and might well excite alarm as to what *Finishers* might perform. Dr. Sanderson explained that it was not for students in medicine, and that its title, "for beginners," "means beginners in *research*" (2240).

Mr. Simon, who, as at the head of the Government Medical Department, employed Drs. Sanderson, Klein, and others, in some "researches," observed of this Handbook, that it was a mistake to "regard it as prescribing a common curriculum for students, where it is in reality [though not in its title-page] *only* an encyclopædia of reference for *experts*" (1500).

So the book might join some of the more eminent experimenters—whose amiable character was declared to be different to what might be harshly deduced from their teaching and practices—in saying, "We are not what we seem."

Indeed, a strict enquiry into the moral effects of the experimenting system might find, in the variety of excuses, evasions, and explanations, advanced in defence, a serious ethical objection to the method which required a resort to these ways of exculpation.

The Handbook was a sort of Bible for English physiologists, who, by its aid, were to lay a new, that is, a "scientific basis" for English medical men.

Dr. Sanderson, deprecating the mistakes into which it had led ordinary readers, who took it in its English meaning, defines its purpose and also defines the physiological school, as distinguished from the medical profession, in these words: "May I make a general observation in reference to this book, namely, that we had *not in view the criticisms of people who did not belong to our craft*, in writing it, and that we did not guard against all possible misunderstandings of that sort" (2265). Here "people who" "belong to our craft," are the physiological school, defined sometimes as those who work in the experimenting way, amongst whom (and whose opinion alone is of force) there was unanimity as to the necessity for *their* services and methods even in lecture-room "demonstrations." "I include, amongst those whom I have represented as agreeing in opinion, those who are actually *engaged* in *research*" (Dr. Sanderson, 2758). None else are considered "physiologists," though physiology is taken to mean the knowledge of the proper functions of organs, which, some might imagine, is as open to medical men and scientific men in general, as it is to the specialists; for if anything is really learned, in the special way of experiment, it may be communicated in "memoirs," or else only experimenters can enjoy the benefit of their discoveries, and in that case they could not fulfil their "proper function" of "holding out a lamp, so to speak, to the physician," or to anybody else.

Besides, other persons not only have the advantage, if it is one, of reading and hearing all that the experimenters

say they have learned from their peculiar line of "research," but they have also every legitimate method of rational and philosophical observation on those cases which we may call *natural experiments*, done FOR THEM in the ordinary course of things—the whole phenomena of life in health, disease, accident, and death, and all forms of investigation after death.

We must, therefore, not allow experimenters to call themselves "physiologists" in any exclusive sense; except in that of persons whose physiology as to man is based on experiments on animals reduced to an abnormal condition by operations, pain, fear, and even by anæsthetics, when used fully or partially.

What they *call* "practical physiology" is properly *speculative* physiology, founded on inferences drawn from practising experiments.

This, then, is the new school, so far as England is concerned; men exclusively devoted to learning about mankind in a normal state, from investigating animals in an abnormal state.

The physiology itself so gained, or as far as gained, is properly organology, or functionology, taken in a detached and isolated state, rather than a complete view of the intercommunication and united constitutional action of the one entire living organisation.

This points out a great defect of the isolated experimenting way, as compared with that which has been called "living pathology," and which takes in the consentaneous action and sympathy of the various parts of one living mechanism.

CHAPTER II.

THE NEW SCHOOL.

MR. HUTTON observed to Dr. Sharpey (538), "that the alarm which has been caused," "has been, to a large extent, caused by the beginning of a *new school of physiology* in this country, in which, besides using these experiments for the purposes of *enquiry*, they use them also for *demonstration*, as seems to be the case with this book [the Handbook]. Do you not think that that is a natural effect of the very large study of our physiologists *abroad*, who are bringing the views adopted abroad into this country very much? I have no doubt that young physiologists who have been studying abroad, in the schools particularly at Leipsic and elsewhere, would naturally be induced to introduce methods followed there, in their teaching in this country."

This new profession, with its second-hand experiments imitated from abroad, has *a different object* and a *different spirit* to that which animates the English medical profession; and which, up till lately, restrained experiments to very practical objects, and was, by its humane character, intensified by being interested in removing human suffering —a guarantee against cruelty.

All this is being altered, under the pretext of supplementing medical knowledge, by a DISTINCT CLASS *exclusively engaged* in physiological speculations which are to give scientific certainty to medical practice, or at least, as the next excuse, to promote abstract science.

After some evidence respecting the cruelties practised

abroad where this so-called physiology has had full fling, Dr. Acland is asked whether "the public feeling in England" is not a barrier to such cruelties here: and he answers—first, that "in the *medical profession*, as far as I know, *that is* the case" (942); and secondly, that this guarantee is now removed by the new school, with its new spirit and new aims.

He says:—"There are, however, biologists now in the country who are *not* medical men, and they are increasing in number. I think that the Commission ought to be aware that the number of persons in this and other countries, who are becoming *biologists without being medical men*, is very much increasing. Modern civilisation seems to be set upon acquiring, almost universally, what is *called* biological knowledge; and one of the consequences of *that* is, that whereas medical men are constantly engaged in the study of anatomy and physiology for a *humane purpose*—that is, for the purpose of doing *immediate good* to mankind—there are a number of persons now who are engaged in the pursuit of these subjects for *the purpose of* acquiring *abstract* knowledge. That is quite a different thing" (944).

After expressing doubts how far this curiosity is a healthy condition of mind, he makes this remarkable observation, that mere professional discoverers of this sort were not the people that gained such beneficial knowledge as we have hitherto acquired.

He says:—"It was *not the way* in which all this mass of physiological and biological learning was added to the world;" but "*by men who were obtaining it for the sake of doing good with it*" "to their fellow-creatures by relieving suffering; *that* was a chief incentive to biological studies. But *now*, it has become a *profession to discover;* and I have often met persons who think that a man who is engaged in original research, for the sake of adding to knowledge, is therefore *a far superior being* to a practising physician who is *simply* trying *to do good* with his knowledge." "Now,

that introduces a *new element*," "and I believe that a great part" "of this difficulty has arisen because there has come to be a pursuit of knowledge in this direction, *just as you pursue knowledge of metals*." "Persons have got to deal with these wonderful and beautiful organisms *just as they deal with physical bodies* that have *no feeling*" (944).

It is with the special line of work—special disposition, objects, and claims of this new class of scientists, introduced from the Continent—that we have to deal.

This *distinction* between medical men and biologists or physiologists, as to aim, and method, and disposition, was acknowledged even by Mr. Erichsen, as a "most important" one; then by Dr. Acland, for the first time, brought before the Commissioners; and since then very little noticed by the public. Mr. Erichsen said:—"981. You made a *most important distinction*, and one that has been made to-day for the first time, before this Commission [namely], *that between medical men and a body of scientific men*, who" "are growing up in this country *who are not medical men*," "who are physiologists and biologists; and it is your opinion that the great" increase of experimenting "is owing to these men working at their science as a science, *independent of its direct application* to the purposes of alleviating human suffering? — I think that" "the pursuit of biology was *formerly generally confined to medical men*," "*whose first object* in life, was the *alleviation of suffering* or the prevention of disease."

The non-medical character of the new school of "professed physiologists," *removed the safeguards* formerly placed around experiments, by the humanity fostered in the very exercise of the duties belonging to the medical profession.

This beneficial moral limit, even to the pursuit of knowledge, by humane means for humane ends, has been exchanged for a metaphysical benevolence, which, in the name of science, finds all means sanctified by its end—

the high profession of the pursuit of knowledge, the first, if not the last, temptation of mankind.

> "Thus ornament, is but the guiled* shore
> To a most dangerous sea."
> "The seeming truth, which cunning times put on,
> To entrap the wisest."
>
> *Merchant of Venice*, Act iii., Scene 1.

This "ornament"—first the pursuit of human benefit, then the pursuit of abstract knowledge, dissevering utility from enquiry, and making "*investigation*" *in general* a special pursuit—is exemplified in "a race of physiologists, "whose business has not to do with the alleviation of suffering, but has to do with research, as to the mode of construction of living beings throughout the world, and all that will elucidate *that*." (982).

Sir Charles Bell, John Hunter, Sir Benjamin Brodie, and others, had, as their "starting point" and motive, "the benefit of mankind."

These attained to real discoveries; "but it is another thing" to pursue investigation "from a simply intellectual standpoint," and apart from the "alleviation of human suffering, as the basis of your aim and character" (983).

The same Dr. Acland, Regius Professor in the University of Oxford, Fellow of the Royal Society, and President of the Medical Council, told the Commissioners, "Physiologists have demanded these laboratories" (978).

It is to the new school that we owe the extended *Apparatus* of machinery for the present laboratories. "The teachers in the department of Physiological Science" "have been endeavouring, as years have gone on, to provide for their students all that they believe is necessary for the mastery of biological knowledge" (980). It was not the medical profession calling out for scientific aid, but the

* "Guiled"—deceptive, treacherous, beguiling.

biologists, infected by foreign fashions, making themselves a necessity, and erecting a *distinct profession* over the heads of the medical faculty, with secrets which nobody but the initiated can understand, and which are so necessary for the good of mankind, as to render innocent all that is inflicted, in obtaining "results" that are often as inscrutable as they are inestimable.

When some of the experiments of the new school are examined in relation to their aid in "the improvement of medicine"—as, for instance, the speculation about the process of inflammation (5205-14) on a point of no possible relation to its practical treatment—this failure to assist in the art of healing is covered by such a question as that of Mr. Erichsen:—5223. "Is it possible to say that any given advance in medical science may not, at *some future* period, have an important bearing upon medical practice?"

Here it was *assumed* that some "advance" *had* been made "in medical science" on a point that was simply an "academical question," and was still debated.

To the enquiry—Whether one could say that some "given advance in science" might not, "at some future period," be useful in "practice?" the answer of Dr. Scott was quite relevant:—"As to such an *abstract question*," it "is very difficult" "to say what is inherently possible or impossible."

This question by Mr. Erichsen (5223) indicates *one clever defence* of the experimenting system—the assumption that it is the *way to knowledge*, and the implication, that those who doubt its *worth* are enemies of "knowledge," or opposed to "science," which is an invidious suggestion, like that of Dousterswivel,* who encouraged Sir Arthur Wardour, and tried to put Oldenbuck out of countenance by the saying, "Art has no enemy but ignorance," which was, in the abstract, an impregnable position, but did not quite logically apply to *his* "experiments."

* *The Antiquary*, Sir Walter Scott.

What is first promised is, "*Knowledge*" *of the way to do good* to mankind, by practical improvement in the art and science of medicine; and *then* we, who were to receive this "useful knowledge"—*i. e.*, useful for *man's sake*—are put off with some assumed esoteric knowledge that is good for *its own sake.*

It is as if Dousterswivel, in answer to the Antiquary's doubts respecting the practical value of his "craft," had said, that if he failed to *promote Sir Arthur's fortune*, which was his original and only profession, he at least promoted *Rosicrucian Science*, which might, "at some future period," be advantageous to mankind.

A long chapter might be written, composed of the protestations, that physiology is *essential for the direct good of man*, through medical improvements, and that physiological science is *itself* a good for man, and also for animals.

Thus in the midst of a series of confessions, that certain experiments had done *no good* to man or beast, but in "some future period," perhaps, might—Dr. Burdon-Sanderson is asked and answers as follows:—"2633. Then you would not say that the race of dogs, for example, had gained by the sufferings to which any particular dog has been exposed in those experiments?—No; certainly not, *not directly*, excepting in the way that I have mentioned just now. *Indirectly the* increase of knowledge *is beneficial to the dog as to man.*"

He would be an ungrateful dog that should, after this benefit, look shyly on a "professed physiologist."

These "indirect" and "abstract" benefits, so far as improving man's condition by the aid of that "Lamp" of knowledge which physiology "puts into the hand of the physician when studying disease" (394; p. 20; col. b., at foot), may appear somewhat indefinite and unsatisfactory.

Their relation to medicine, and through it to man, is described as *only one* of *the incidental* results of a science

which, originated for man's good, exists for its own glorification and that of its professors.

Thus Dr. Sharpey, an intelligent and enthusiastic admirer of the system, puts the *value of the* SCIENCE *far above* the *value* of its RESULTS; which, indeed, is the only way of saving its credit after all the promises of conferring real benefits made in its behalf. He is asked, and answers as follows (398):—"And it is your opinion that the *importance* of experiments of this kind for *scientific* investigation *is very much greater than* for *any direct medical or surgical results?* Decidedly. I believe the influence of physiological knowledge upon medicine is one that *may not be very conspicuous*, but it is not the less true. It operates *perhaps impalpably* sometimes upon the mind of the practical physician. In short, I should say that physiology is not to be compared to the *reaping* machine, but rather to the *plough*."

This style of language, so common when definite instruction is needed, is perhaps what Mr. Huxley describes in his preface to "Physiography, an Introduction to the Study of Nature," as possessing "that precision by which science is distinguished from common information."

It is quite true that "physiology," as assumed by the experimenters, is "*not* to be compared to the *reaping* machine;" it gathers nothing from the "field of research" that will serve either for food or physic; but it may be compared to the "plough" by those who feel the "share," and who, equally with men, share "indirectly" the "beneficial" knowledge so "turned up." We are several times informed that experimenters do not quite know how far *Curari* admits of suffering, *because* the animals "*cannot speak*" (436); if they could, they might say of this physiological plough and its drivers—"The plowers plowed upon my back; they made long their furrows" (Ps. cxxix. 3).

Man also may have a word to say in acknowledgment that for "direct medical or surgical results" the infiuence of

physiological knowledge upon medicine is one that is "*not* very conspicuous;" and that if it operates at all, "it operates *very impalpably* sometimes upon the mind of the physician;" and *this* at least is an advantage, since Dr. Burdon-Sanderson, one of our chief experimenters, advises that physicians should have very little to do with "practical physiology," because their "*experience*" is worth more than *experiments* (2731).

This confession is so extraordinary, so thoroughly subversive of the value to medicine, of the "craft" of experimenting physiologists, so contrary to the frequent protestations of its advocates, and withal is so frank, true, and important, that we give it here, though it might properly fit in with another line of argument and observation. The following is the question and answer (2731):—"But as regards the higher consulting physicians and surgeons, you would wish to see a larger number of them passing through these laboratories, and you would *not think them competent to advise on the more difficult cases* WITHOUT IT? *No*, I would *not go so far as that;* because, *as medicine stands at present*, it stands ON EXPERIENCE; and the man who is *most competent* to advise, is the man of *most judgment* and *most clinical knowledge;* in other words, of MOST EXPERIENCE."

CHAPTER III.

THE THREE PRIMARY ASSUMPTIONS; OR, TRIPOD OF ERRORS ON WHICH "PROFESSED PHYSIOLOGY" RESTS.

THE following three assumptions, in words or meaning, will be found running through the defences of experimentation:—First, that physiology is the basis of medicine; secondly, that experimentation is the basis of physiology; and thirdly, that only experts in these experiments can judge their value.

These three propositions are but the logical unfolding of what is constantly implied.

The claim in effect comes to this—physiology, meaning this particular method of pursuing it in animal disturbance, is the only foundation for that human physiology on which scientific medicine is to be erected; and of this claim only those "accomplished in [such] physiological matters" are competent judges.

This—to use the favourite, if not the cant, phrase of the psychological workers on the brain—is "the unconscious cerebration," or the implied and sometimes expressed method of argumentation, adopted in defence, by the so-called physiologists.

The statement is as groundless as it is bold and confident.

The third of these propositions—which founds the claims of a particular science on the general ignorance, and requires the world to believe it because only its professors understand it—is discussed under the head of the "survival and extension of pain, after its professed extinction or reduction to an infinitesimal quantity by anæsthetics" (Chapter x.).

We, therefore, here note only the first and second propositions which are essential to the claims of the experimenters.

First. That physiology is the foundation of medicine.

Here, without fear of being scorned for ignorance on matters esoteric to the specialists in physiology, we may say a few words on the nomenclature or terms used in medical science. These, in general, are very definite, and have an *etymological meaning* which, to an educated person, indicates the general application.

Ætiology, as referring to the causes of disease, from *aitios*, as implying an *injurious* cause, used as the Latin *reus*, for defendant, accused, or prisoner—the rogue that did it; and hence Ætiology, or an account of causes of *evil*, is instructive; so with Pathology, an account of sufferings, from *Pathos*, is readily understood; and Therapeutic, as meaning that which heals, is another word transferred directly from the Greek, and is equally instructive; and is *the response to Pathology* as a remedy for the suffering which that word implies.

But "physiology" has no such definiteness, and is merely arbitrarily used. Properly, *Phusis* is *Nature*, and is as wide as the universe; like "*Physiography*," a description of Nature, as used for the head title to Professor Huxley's "Introduction to the Study of Nature."

"Physiology," however, is applied *arbitrarily* to the functions of organs; and, as already observed, as so used, it properly means functionology, being used to denote the science of the intended or proper work of definite parts of the body.

The work of the external organs—eyes, ears, nostrils, and palate—called "the senses," is known to the commonalty, and also that of some internal organs, is a matter of "common information," while a more intimate acquaintance with their functions and derangements belongs to the medical profession.

Now it is *only with derangements*, in order to put them right, that medicine is directly concerned. But, because "physiology" is used for the undisturbed functions, it is argued that a knowledge of *this*, is an aid towards restoring

a disordered organ to its proper condition. This is like saying, that a knowledge of what the eyes are for, will help us to cure weak sight. Whereas it only tells us that, if a man is nearly blind, his eyes are the immediate organs affected; but it does not help us to a remedy. Perhaps the eyes themselves are affected by other constitutional disturbances, as cold, weakness, or general debility.

But a knowledge that it is the eyes that are affected, when the sight is imperfect, is no news of a remedy.

Now, *medicine has to do with remedies;* it deals with the pathological condition, and no amount of physiological knowledge aids beyond telling us the seat of the disease.

It is therfore *impossible, from the nature of the case,* that physiology, which is only a knowledge of the functions in health, can be the foundation of medicine, which deals with the functions in disease.

Therapeutics belong to Pathology, and *Therapeutics are medicine;* and they are learned, not from physiology, and never can be learned from it.

Physiology may explain *where* the evil lies, Ætiology may tell *what did it,* but neither, nor both of them, can tell how *to cure* it; *that* is left to the skill of the medical practitioner, considering what remedies have been proved by experience, and what may be inferred by analogy.

This is what Sir Thomas Watson testified, that a knowledge of functions, as of those of the nerves, might *indicate the parts* affected, and so "enable us to *direct* our therapeutic influences, the influences of drugs, (which [influences] have BEEN ASCERTAINED IN FAR OTHER WAYS) so as to soothe pain" (35). Here he admits the use of a knowledge of the functions of the nerves, as, in that case, telling the physician what parts to aim at with his remedies.

But it does not, in any case, provide or discover remedies; these, he tells us, "have been been ascertained in far other ways," namely, than physiological researches.

Dr. A. de Noé Walker proved distinctly that physiology,

as a knowledge of functions, in no way indicated any remedial agent, though it might, "in many cases," "not in all, determine the seat of the disease" (1721). He said: "That the knowledge of the healthy functions of animal life does not contain in itself any knowledge that can lead us to the therapeutic agents for the treatment of any disease by which those parts may be affected" (1721).

In the same statement he pointed out that nobody, from knowledge of the healthy functions, ever deduced or discovered any remedial agent.

"Physiology can only help us, occasionally, to determine the seat of disease in the living subject, and there its use in medicine stops." This "localisation of disease," "is very much limited to the nervous centres" (1739). "You may know the functions of the bladder well, but you would not recur to a knowledge of its functions to find out what disease affected it" (1740).

Dr. de Noé Walker also quotes one who is a great friend of the physiologists, and who earnestly advocates their freedom of experimentation, as saying: "The clinical physician knows that the phenomena of disease are not explained by 'the phenomena of healthy texture,' nor by the action of healthy organs" (1749).

So physiology neither explains the disease nor finds a remedy, but only sometimes indicates the seat of the malady (see also questions 1758–9, 1780).

It was this line of argument, by Dr. Walker, that led Mr. Erichsen *to give up* the doctrine, that physiology is the foundation of *medicine*, and to confess that it dealt only with health, and could do nothing for disease.

Need we say more here on this than quote the confession? It is as follows:—"1776. You mean, I suppose, that there is a class of men who are devoted now to the study of the structure of animals and the functions of their organs, quite irrespective of medical practice? Yes.—1777. But IT IS NOT THE BUSINESS of those men *in the study of the healthy*

organisms, TO DISCOVER NEW METHODS OF TREATMENT, or to devise new plans of operations, for instance; their *business is simply to* DISCOVER *the actions* of the bodies of animals *in the healthy state?*—And it would be very important *if they kept to that*, but my teachers always *misled* me for some years *by saying* that *the knowledge of the healthy* functions, which they were teaching us, *was to form the basis of therapeutics* and of pathology."

Thus the first and leading assumption, that physiology is the foundation of medicine, collapses; and therefore the *second* assumption, that experiments are the foundation of physiology, is rendered *useless*; like "supports" that come up after the battle is lost. Whether physiology has sprung from experiments, or not, may be noted again, if we have space to examine some of its alleged "discoveries;" but at any rate we have destroyed this alleged "benefit" on which its *continuance* is claimed—that it is for "the improvement of medicine" by giving to it a scientific foundation.

We have already expressed doubts as to the special possession of "physiology" by those who claim to *monopolise* it, and may dismiss that point with the sceptical hint given by Dr. Rolleston, who seemed unwilling to recognise "physiology" as only another name for experimenting. He said:—"I do not profess to teach experimental physiology, that is to say, *if physiology is to mean* what it is made by some professors of it on the continent, namely, *nothing but vivisection*" (1343).

Thus at every step we have to guard against assumptions of matters *not scientifically proved*, but claimed confidently, without any basis, by those who claim "science," "proof," and "exactness of knowledge," as their peculiar distinction; and whose "function" is to communicate this scientific precision to the province of the medical faculty.

Mr. Erichsen, however, fairly gives up the claim, and confesses "it is not the business of these men" to find remedies or direct medical treatment (1777).

CHAPTER IV.

PHYSIOLOGY NOT BEING THE FOUNDATION OF MEDICINE, THE PHYSIOLOGISTS NEXT FIND A SECOND SCIENTIFIC BASIS FOR MEDICINE, BY MEANS OF UNSCIENTIFIC AND UNPHYSIOLOGICAL EXPERIMENTS IN PATHOLOGY.

THE Commissioners in their Report (ix.) say:—"Medicine rests on the *triple basis* of clinical observation, and pathological and physiological *research*." We have seen *one* of these three supports—Physiology—adopted as *the* foundation, and abandoned as unstable; now we have another of them selected, namely, Pathology. Thus Professor Humphry having acknowledged that without experiments "our knowledge would be in a *state of comparative* barbarism" (685) next indicated that the experiments are to be pathological, and not physiological. Thus, after saying that mere "clinical observation"—one part of the Commissioners' "triple basis"—and "observation in the dead house," would "leave us entirely ignorant of the processes;" he is asked, and answers as follows: "687. The *vital* processes? Yes, the MORBID process, on a *knowledge of which* AFTER ALL scientific *medicine must be based*."

Now this "*morbid* process" belongs to pathology; the *healthy* process belongs to physiology.

These pathological experiments constitute a large proportion of what is miscalled "physiological research," and it is acknowledged that these experiments are "*not* scientific;" yet, "after all" said in favour of physiology as *the* method of removing the "empirical" character of medicine, and giving instead, a "precise" "rational basis," *that basis* is to be given by these unscientific and unphysiological processes.

All the experiments for infecting animals with diseases, all toxicological experiments, and the operations for testing

the action of drugs on the livers of dogs or cats, are pathological. They have to do with diseases, and not with healthy functions.

In reference to all that useless class of experiments, "with a view to elucidating" in animals "the morbid conditions that do occur in the human subject," especially settling the *physiologists' difficulty* respecting "biliary secretions" (5323), at which they have worked so long for nothing, Mr. Erichsen treated *the whole as unscientific;* and yet they are a portion of those experimenting researches by which a scientific certainty is to be given to medicine. Thus the action of rhubarb on the liver "has been *suspected,* but not proved." But *now* "we know *definitely*" that rhubarb "*is* a powerful stimulant of the liver," *i.e.,* in dogs (2904).

But all this "definite," "certain knowledge," as to dogs, of what was "only suspected" as to man, is acknowledged *not* to be "scientific."

Thus Mr. Erichsen, addressing Dr Legg about his experiments of this sort, said: "5320. *(Mr. Erichsen.)* Your experiments have *chiefly* been of a pathological or therapeutic character, RATHER THAN of a *strictly scientific physiological* nature?—Yes." "5321. They have all been conducted, I presume, with a *direct bearing on* the elucidation of some point in practice, or *treatment;* some point connected with disease that may occur in the *human* subject?—Yes."

Now, here are our physiologists seeking, by pathological, unscientific, and unphysiological processes, to give a scientific basis to medicine.

This is the *second* "rational" foundation to which physiologists have shifted the healing art. The first scientific basis is on the healthy functions, or physiology; the second, is the "morbid process," or pathology. They "advance the science of medicine" by placing it in that undesirable position,—"between two stools,"—so that, according to the

proverb, it must "come to the ground" if it have no surer basis.

What sort of scientific basis, all the "unscientific" pathological experiments of the physiologists, give to medicine, is fully indicated, if not confessed, by Dr. Rutherford; who, being asked whether "in the experiment" "referred to"—respecting biliary secretions—he considered that he had "*established* the *fact* that the action of rhubarb on the liver is to increase the flow of bile?" (2961) answered honestly and logically, "Yes, in the case of *the dog*." That is, he did not pretend to have "established the fact" "in the case of" MAN.

The perpetual source of fallacy, vitiating all the physiologists pathological experiments, is, that a drug may not only have different effects in kind and degree on different animals of the same species, and on those of different species, but may have a different effect on man from that of its influence on all or any of those animals. This, which makes it *impossible* for these experiments to give "scientific" certainty even respecting animals, makes it still more absurd to transfer such results to medicine as scientific guidance in the treatment for human beings.

That "after all" the experiments on animals, the medical practitioner is *to try the experiment on man* before any conclusion is to be drawn, is confessed by Dr. Rutherford as follows:—"The experiment must *also be tried on man* before a conclusion can be drawn" (2966). As to "what would be the effect on man?" "No conclusion can be drawn till the experiment is tried" (2967). "You leave it to the medical men to determine whether they will use the rhubarb which you have shown has the effect in case of the dog?" "YES. I show *the fact on the* DOG, leaving it *to others to experiment on* MAN" (2962).

This is a frank and logical abandonment of all certainty given to medicine by the pathological experiments of practising, not "practical," physiologists.

CHAPTER V.

THE SPECIALITY OF PROFESSED PHYSIOLOGY IS ITS UNCERTAINTY, BOTH AS TO ITS "RESULTS" OR "ESTABLISHED FACTS," AND AS TO THE USES OF ANY OF THEM.

"I THINK," says Professor Humphry, "*pathological* experiments are, and will continue to be, very important and very useful for the welfare of mankind." . . . "Indeed, I cannot help thinking that the *scientific knowledge* of medicine is to be based to a *very considerable* extent on *experiments on animals*" (611).

But the same authority says:—"We all know how *very difficult* it is to *ascertain a fact*, and those *experiments* on living animals are *associated* with observations of *very complicated* facts; they are often *very difficult* observations, and therefore they have to be *repeated* and *confirmed many times* before the fact is really ESTABLISHED" (635).

He might have added, that when a fact *is* "established"—which it never is, so long as any physiologist thinks it should be "repeated for verification,"—it may at any time be *dis*-"established," like those famous speculations on tuberculosis, in which our "home-bred physiologists" have so extensively and uselessly copied their foreign masters. Thus Dr. Hoggan testified:—"According to the very latest information" "from the Continent, the experiments and conclusions *which are most relied on*—those of M. Villemin—have been *completely upset* at the late Medical Congress at Brussels, by Professor Crocq" (4134). "I simply bring it in to show that *those* experiments, *which* were *supposed to have justified* all experiments on animals on this question, have been upset by the very latest investigations upon the subject." "M. Villemin in his letter [to the *Revue*

Scientifique] acknowledges that he has modified his ideas, and that the WHOLE FIELD STILL LIES OPEN *for work*" (4135). This may be said of all "the fields" in which these experimenters have used "the plough."

This question of tuberculosis, nursed so carefully in the evidence and questions of the Commissioners, as giving importance to experimenting, on the illusory ground of the wide and fatal effects of tuberculosis, *as if* they were on the *road to a remedy*, is *only one* of those instances in which "the whole field is still open." Animals are tortured, and man is not relieved.

Mr. Huxley tried manfully, and with his accustomed skill, to find some advantages in the broken pieces of this great experimental wreck, but the witness was firm and clear:—"They have only contradicted each other. I am not aware that they have come to really a better knowledge of what tuberculosis is, than they had 30 years ago" (4139). This knowledge of "what a disease is," is one of the objects of "research;" it aims at some special insight, "almost supernatural" (1764), not yet attained, and which, if attained, would not show *how to cure* the disease.

Anyway, here is the speciality of the certain science of physiology,—that even its experimental results are uncertain, and are always being set up and "upset;" "established," "verified," and disproved. A whole history of its failures would fill a large book; its therapeutic advantages would go into a small foot-note; and then would be open to fatal criticism.

The most boasted discoveries credited to the system by its advocates, as Sir Charles Bell's discovery of the two-fold functions of the nerves—which everybody by feeling and motion might perhaps have "suspected" the nerves had—although *again* "*established*" by Magendie's experiments, may yet be tried for "verification;" "forasmuch as there have been *complications* even in that very experiment, it has *not* been quite *so simple* a matter as may be supposed,

nor have the *inferences* drawn from it. I do not think that the SO-CALLED ESTABLISHMENT of the facts by two observers (Sir Charles Bell and Magendie) would *even in that* instance have been sufficient to render further experiments unnecessary. That, however, was ONE OF THE MOST SIMPLE CASES" (636, Dr. Humphry).

Now, this was a "physiological and purely scientific" experiment and "discovery;" yet is open to trial as only "suspected." How much more, then, the pathological experiments, and the *transference* of their *inferences* from cats, guinea pigs, dogs, mice, and monkeys—to man!

When an attempt was made to find a limit to repetition or *re*-search, for what had often been found in the experimenting way, Dr. Humphry replied:—"It is *very difficult* to know when a fact HAS been established" (680). This he has to repeat:—"I think it a very difficult thing to state when a fact *is* established" (772).

The curious thing is, that physiologists still insist on calling these non-established lucubrations — physiological "*facts*."

"Now," observes Dr. Gamgee, as a reason for continuing this kind of search more fully, because it finds nothing, "PHYSIOLOGICAL EXPERIMENTS are MORE LIABLE TO FALLACIES THAN ANY OTHER experiments; there are so many *circumstances which belong to the particular animal* which is being experimented on, which may lead to a fallacy in judgment, that it is *most essential* that *many* observations should be made" (5390).

Dr. Burdon-Sanderson observes:—"If you take the *progress* of physiology, I mean during the past 25 years, there is no doubt that IT HAS CONSISTED, in a great measure, IN REVISING statements of *facts* (!) which had previously been *ascertained* (!) by less exact observations" (2781). All the alleged discoveries by which systematic experimentation, as the entire work of an exclusive profession is defended, were made by "the less exact" method; and even now, with

these undefined more "exact" pretensions, the profession is occupied in framing and disproving *theories* of "practical physiology."

The Commissioners refer with delight to the phrase, "a great scientific revival," claimed by Dr. Turner (3090); but it is only a great scientific REVISAL; in which, as wrestlers in a match try to throw one another, the "professed physiologists" overturn each others' "established" positions, which is called "revising statements of facts;" and which only illustrates how "very difficult a thing it is to know *when* a fact is established."

But while uncertainty is the speciality of physiology, it is reassuring to find some practical if not scientific certainty in the methods open to that medical profession which is to gain its only scientific basis from the fluctuating clouds—"driven about by every wind of doctrine"—issuing from the caverns of "practical physiology."

Sir William Fergusson is asked and answers as follows:—"1103. And in your opinion, is clinical observation and pathological observation of more service to practical surgery than experimental physiology?—Yes; there is *precision* in the one, whilst the other is *largely theoretical*."

CHAPTER VI.

PHYSIOLOGY HAVING NO CERTAINTY ITSELF, SCIENTIFIC OR OTHERWISE, UNDERTAKES TO GIVE SCIENTIFIC CERTAINTY TO THAT WHICH, FROM ITS VERY NATURE, EVEN AS CONFESSED BY PHYSIOLOGISTS, IS INCAPABLE OF RECEIVING IT; THOUGH IT HAS A PRACTICAL CERTAINTY OF ITS OWN, SUCH AS IS RELIED UPON AND ACTED UPON IN ALL HUMAN AFFAIRS.

THERE is a kind of assumption running through physiologists' references to medicine, namely, that we can understand in some recondite sense the *reason why* certain things operate in their special ways; and that *practical knowledge* of actual effects is, in some derogatory sense "empirical." Now, in truth, this is *all* that any one *can* know of *anything*.

For instance, strychnia is what is called a "spinal poison," as Dr. Taylor tells us (1174), and no living creatures without a "spinal marrow" are affected by it. "It requires a certain organ to be affected by a certain poison." It is *only observation of the fact* that tells us this; it is *not reasoned out* from the nature of things; it does not follow as a mathematical conclusion from ideal mathematical data. Therefore, to pretend to any other kind of knowledge on these matters is *sciolism, and not science*. Scia, a shadow, or darkening, is not connected with science, it does not etymologically mean even a little knowledge, but is responded to in the description—he "that darkeneth counsel by *words without knowledge* (Job xxxviii. 2).

It was said, for instance, that "doctors go on giving blue pill *just* as before," which "practice" is said to be "*purely empirical*." "They find that" it "does" a man "good," and "they call it relieving his liver, but whether it relieves his liver or not, they do not know." "They have *simply*

observed that giving blue pill under such circumstances *does good.*" This is Mr. Huxley's reasoning (1934-6). He seems to think, first, that experiments are *not* "empirical," though this latter term is English—Greek for an experimenting way. He also imagines we may have some *other kind* of knowledge of these matters than "simply observing" that certain agents "do good." Whereas, all we know of any poison is that we observe that it kills; or of any therapeutic agent, that we observe that it cures. *Why* the poison kills, why the other cures, all the Rosicrucians that ever dreamed could not tell.

All outside this is fantastical; it is like a saying in the preface to "Physiography," that a person cannot have a proper idea of the affairs of "his own parish, or of a pebble" —"what" the latter is, "and why it is, where it is"— without some acquaintance with chapters of our earth's history, and of the universe, untold ages ago.

All this knowledge would not make "the pebble" any harder, nor the object at which a villager might throw it, any softer; it explains nothing; as the logician, J. S. Mill, would say, it only surrounds a "familiar mystery" with unfamiliar ones.

And if, in the title of the book "Physiography," the author retained the Greek u, instead of substituting the English y, it would indicate phonographically the kind of knowledge that is liable to pass for that "precision by which science is distinguished from common information."

The pretended scientific explanations of the *reason why* are well described by John Stuart Mill in the first volume of his Logic, pp. 559-560:—"What is called explaining one law of nature by another is but *substituting one mystery for another;* and does *nothing* to render the general course of nature *other than mysterious.* We can no more assign a *why* for the more extensive laws, than for the partial ones. The explanation may substitute a mystery which has become familiar, and has grown to *seem* not mysterious, for

one which is still strange. And this is the meaning of explanation, in common parlance." . . . "The laws thus explained or resolved are sometimes said to be *accounted for;* but the expression is incorrect if taken to mean anything more than what has been already stated. In *minds not habituated to accurate thinking* there is often a confused notion that the general laws are the *causes* of the partial ones; that the law of general gravitation, for example, causes the phenomenon of the fall of bodies to the earth. But to assert this would be a misuse of the word 'cause;' terrestrial gravity is not an *effect* of general gravitation, but a *case* of it; that is, one kind of the particular instances in which that law obtains."

This larger and more philosophical view of things may prevent scientific physiologists any more throwing scorn on medical practice for being founded on "simply observing the fact" that any particular agent "does good."

When we "observe" *this* of physiology, we may give it credit for more than promises.

Speaking of parasitical diseases, Dr. de Noé Walker observed:—"Kill the parasite, and the disease is cured, just as a surgeon removes a splinter from the soft parts, and cures his patient" (1769). To which this answer, by way of question, was given:—"1770. By the rule of thumb, you mean?" Now, by what other *rule than "the rule of thumb," aided by fingers and forceps,* must a splinter be removed? What scientific reasoning or explanation would be needed or able to show the propriety of removing it, or improve the process of taking it out? and what physiologist ever "discovered" that sulphur would kill parasites, as in the case of ringworm?

We see what the assumed scientific knowledge or elaborated ignorance, respecting tuberculosis, has done in a physiological way; it has experimented on what is called the "real nature" of tuberculosis, where, with many recondite terms as to the use of our knowledge, that is, "*as*

soon as it is complete," we learn only one thing, namely, that they have discovered it is "*incurable.*"

This is their helpful "knowledge."

We are always thus put off with quasi-scientific jargon, and told that some day they may yet find out how to prevent, what they have not found out, how to cure; but it is all done in a scientific and accurate way, and that is our only consolation.

Every subject has its own peculiar methods of evidence, suited to its own character; to pretend to apply mathematical reasoning to business or to sociology would be empty pedantry; and those who acknowledge that medicine is *not* a science, "LIKE mathematics or chemistry," should not pretend to apply to it mathematical processes.

Dr. Burdon-Sanderson fondly hopes for that which a "brother physiologist" proves, *from the nature of the case,* to be *impossible.*

The hope is, that physiology, whose specialty is uncertainty, will yet give a mechanical certainty to medicine, which is incapable of receiving it, because *its subject matter* is neither mechanical, nor chemical, nor mathematical.

Overlooking all this, Dr. Burdon-Sanderson says:—"It is my profound conviction that a *future will* come, it MAY be a *somewhat* distant future, in which the *treatment of disease* will be *really* guided by *science.* Just as completely as *mechanical* science has come to be the guide of mechanical arts, do I *believe* that *physiological* science will EVENTUALLY *come to be* the guide of medicine and surgery (2704)."

Now, as physiology is an account of healthy functions, and medicine has to do with diseased functions, they are plainly separate provinces, as already proved; and, as diseases are infinitely various, according to the varying constitutions and conditions of men, they must always, *if treated intelligently,* be managed by intelligent adaptation to the case in hand, rather than by any "rule of thumb" of mechanical or other science foreign to the matter.

This affectation of *scientific* precision, unsuitable to a *practical* subject like medicine, is the more inconsistent on the part of those who know and *say* that it is utterly inapplicable.

What they are professedly seeking to give to the healing art is what, after all, they confess is impossible.

Two chemical elements enter into definite relations; but any therapeutic agent, tried on a living body, is dependent on the special conditions of that particular body, and is a matter for judgment and experience, not for a "rule-of-thumb" process, which may serve for definite chemical combinations, or for an ordinary sum in arithmetic, which is done rather mechanically than from special thought and careful inference.

This is acknowledged when the professedly physiological, but really pathological, experiments on the perpetually persecuted liver of animals, is brought to the test of utility. *Then* we have this confession, as an excuse for the experimenter's failure, to give scientific results:—"2959. I must remind you that MEDICINE CAN never be PERFECTLY EXACT" (Dr. Rutherford). Why, this is what physiology was to *make* it, and now it is confessed to be impossible.

Hence, by way of apology for *not* giving scientific results, Dr. Rutherford says:—"We [experimenters] shall *never* be able to say, that in two *different individuals*, with livers in a state of torpidity, from *different causes*, that the same dose of rhubarb or of podophylline will produce the same effect, *or* even, *whether in any case*, they will produce the *same marked effect as* they do *in dogs*" (2959).

So the fundamental pretence of giving scientific accuracy to medicine, and on which "professed physiologists" base the claims of their "craft," is a confessed impossibility, as inconsistent with the very nature of medical work and requirements.

CHAPTER VII.

THE FOUR CONDITIONS GENERALLY RECOGNISED BY PRACTISING PHYSIOLOGISTS AS ESSENTIAL FOR A JUSTIFIABLE AND USEFUL EXPERIMENT — THE FIRST THREE BEING INDEFINITE EXCUSES FOR UNDERTAKING IT, AND THE FOURTH BEING A PROVISION AND APOLOGY FOR FAILURE.

THERE are four points recognised as of importance in considering the propriety and value of an experiment:—

1. Its purpose, or the end intended to be served.
2. The scientific probability of securing the end.
3. The amount of suffering inflicted, compared with the value of the purpose, and the probability of attaining it.
4. The logical reading, or interpretation of the facts, observed in the process.

This scheme will show the futility of many of the excuses put forward in defence of the system.

1. As to the purpose: it must be (*a*) definite, (*b*) important, (*c*) for the good of man, or (*d*) for the advance of science.

These are the forms in which "the purpose" is defended.

Any or all of these four qualifications of "the purpose" may be dispensed with at convenience.

We will notice them in turn:—(*a*) A definite purpose.

Lord Winmarleigh, relying on the idea that these representations about "the purpose" were realities, and not pretexts, inquired of Dr. Foster whether one "engaged in physiological research should be bound to state *what experiments* he proposed to make, and *what the object* and *result* of those experiments were, before he was allowed to

perform them?" Dr. Foster replied:—"I think he could hardly do more than indicate *the line* of his research. *Because*, directly you begin an enquiry you begin *in one line*—you begin, I mean, with one set of experiments; and at once something occurs which takes you off in a completely different direction. No one can possibly declare completely beforehand what are the experiments that he wishes to make" (2395).

The absence of "a *definite* purpose" means the absence of any real object, except the general "experimenting idea."

The physiological profession is above being confined to a definite purpose. Sometimes the excuse is,—we never know what may turn up.

Dr. Burdon-Sanderson, after some severe criticism on "people who spoke vaguely," gives this vague definition of the "purpose":—"Any investigation which is done *bona fide* for a scientific purpose, *for the purpose of investigation*, would be a proper investigation to make" (2314). Nobody can make head or tail of this:—"Investigation done *bona fide*" "for the purpose of investigation, would be a proper investigation to make." He *defines* the word *by itself;* but what "investigation" *is*, or what it is *for*, is not indicated.

(*b.*) An "important" purpose, is another excuse; we have abundant proofs that "what *is* an important purpose?" is an unsolved problem.

(*c.*) "For the good of man" is too wide a profession; for if "the good" proposed is *nameable*, it comes under "definite purposes."

The same is true of (*d*) the "advance of science;" any scientific object could be distinctly stated, and so come under (*a*) definite purposes.

It should be noted here, that the distinction commonly kept up between "scientific" and "beneficial" results is occasionally discarded, in order to *make science* itself beneficial.

These benefits of science are acknowledged to be in-

direct, as we are gravely told by so high a scientific authority as Dr. Burdon-Sanderson: — "Indirectly, the increase of knowledge is beneficial to the dog as to the man" (2633).

No doubt the sort of knowledge referred to is quite as advantageous to the one as to the other. This answer was given as an *excuse for the failure* to attain any *knowledge that would cure* the diseases of animals; for the efforts had *promoted science*, and *that* is as good for "the dog as for" his master.

So much for that autocratic "purpose" of the experimenting physiologists, which is advanced in justification of any sufferings necessary for its gratification.

2. The scientific probability of the purpose being secured by the experiment. If this "probability" is founded upon the proportion of misses to hits, in all the promising experiments that have been performed, it would be a very poor chance.

Sometimes it is put in this way:—painful experiments are justifiable "when there is a *reasonable* prospect of his [man's] condition being benefited by it" (599. Professor Humphry); or when "it can be *shown* [to whom?] that they are *likely* to promote the benefit of man" (601).

Now, looking at the nature of many of the experiments, and the known failure of those and of more, it is reasonable to conclude that they are NOT "likely to promote" human benefit, however well adapted to engage curiosity.

Even Mr. G. H. Lewes, who regarded "vivisection" as "a painful necessity," said: "A great deal of experiment is quite useless; useless because it very often *could not* prove what is attempted to be proved" (6330).

As to the probability of an experiment securing an end beneficial to man, Dr. A. S. Taylor, experienced in toxicology and medical jurisprudence, was asked: — "1175. Then it requires a great deal of scientific knowledge to form a just

opinion whether a particular experiment in toxicology will be of any use or not?—Yes."

"1176. Use for purposes, I mean of application to the human subject?—Yes."

"1177. Either in regard to medicine or to jurisprudence? —Yes."

The first two rules, requiring a beneficial "purpose" and a "reasonable prospect" of answering it, are sadly undermined by the following question to Mr. G. H. Lewes, and his answer:—"6331. In point of fact, it requires a person of very great powers of mind really to form a just conception of whether an experiment will be of service or not? —Decidedly; and even then, he can do it only with a very obscure sense of the probable results."

This also adds another rule—viz., in the qualification of the experimenter, which no license can give him—"very great powers of mind."

3. The third requisite towards justifying an experiment is, to consider the amount of pain inflicted in comparison with the value of the purpose, and *the probability or improbability* of *obtaining* it. The latter part of this rule, "as to the probability or improbability," is generally, if not always, omitted by the defenders of experimenting physiology, who parade *the value* of the result *aimed at*, and thus *puzzle*, if they do not convince, the simple enquirer.

Thus, as to the cure of snake-bites, by the attempted discovery of an antidote, the Commissioners put it strongly, tenderly, and repeatedly:—What are a few hundreds of animals compared with 20,000 of our Indian fellow-subjects who die annually from snake-bites—should we not willingly sacrifice the animals to save the human beings? Mr. Colam very coolly spoiled this *ad misericordiam* fallacy, by telling them flatly that he did not believe the sacrifice of the animals *would* save the people. (1689–1692.) The purpose here is to *indicate the fallacy* of speaking about certain

results *aimed at*, and omitting the improbability of ever *reaching* them that way.

Even Mr. Erichsen admitted (1690) "No result has *as yet* been reached."

If, then, we compare the sufferings inflicted, with the *improbability* of the end being attained, we should state the problem more honestly, if not more scientifically. This also brings all the cases, say millions, of the animals that have suffered for "no result as yet," in all the different "purposes" of these researches, and puts all this agony in one scale, and the "no results" in the other.

The *suffering* in each case is *a certainty;* the grand result desired, is *not* even a *probability*. Comparing any one experiment about to be tried, with the numbers that have been done for nothing, as the result, the chances are a million to one against any benefit resulting from that suffering.

4. The fourth requisite to secure any *possibility* of advantage is the logical reading or interpretation of the facts observed in the process of the experiment. This is the most difficult part of the research: anybody may *see* what happens or goes on, but not one in a million could say what it meant, in relation to any physiological, or, as is more general, pathological enquiry, or to any therapeutic advantages.

The vice of this experimenting system is, as is confessed, that it is more open to fallacy than any other line of experimenting; and the reason is, because such experimenting is not suited to physiological research.

What are "the results" that have been accumulated by those who have presumed to interpret experiments? They are the opprobrium of science. This is why so many "discoveries" have to be so repeatedly "verified." Mr. G. H. Lewes thinks some restraint is necessary to prevent the increase of the mass of "rubbish" which they dignify as

"facts," and that "for the sake of science it ought to be restricted, so as not to encumber science with *useless lumber* which *the mass of it is*" (6345).

On the whole, there is the utmost uncertainty in this special line, which is to give certainty to medicine. Certainty is the point that physiology of this sort most needs.

The delicacy of perception required in reading an experiment is thus described by Mr. Lewes:—"The organism is so complex that, when you produce even a slight disturbance, you are seldom certain of what *other* disturbances you produce; so that an experiment, which seems quite decisive by the phenomena it presents, will turn out to be totally indecisive, because the same results may be obtained by a totally different experiment."

Dr. H. W. Acland, noting the rare qualities required for reading an experiment, and interpreting it with any accuracy or advantage, declared: "Even for us, who spend our whole lives in it [experimenting], it is very difficult to follow the intricacy of some of the inquiries" (952).

"It is a very rare quality, that of being fit to do these things; it is a very rare thing, even for a medical man, or for a professed physiologist, to be able to pursue this subject to any advantage to mankind" (953).

CHAPTER VIII.

THE RETREAT INTO HISTORY; OR, THE DEFENCE OF THE PRESENT METHOD OF PROFESSIONAL EXPERIMENTATION, ON THE GROUND OF PREVIOUS DISCOVERIES.

WE have already expounded, if not exploded, the scientific and logical claims of the working experimenters; and we have now to deal with *the excuses and apologies* of a popular character, to which the advocates of the system resort as a kind of foreign outwork and defence. Nothing is more plausible nor more fallacious than the appeal to alleged discoveries, as made by the system, but which were made *before* the system of professional experimenters came into vogue.

(1.) First, those who resort to it for popular effect, deride the judgment of the populace as to the value of their work, as we show in a subsequent chapter. *None but experts can judge;* and so the public is warned off from presuming to form an opinion respecting matters so recondite as physiological processes, and even as to the value of their results.

Yet with this high contempt for "lay" intelligence, the advocates of *systematic professional experimentations* condescend to appeal to *the public* on points which *are* outside the knowledge of the public in general, and, perhaps, of the "experts" in particular—namely, on matters of special history, in the biography of individual discoverers.

This appeal to the general public, on points requiring special historical knowledge, is a suspicious resort for those who decry the public judgment, and even that of "well-educated persons," as unfit to judge of their "craft" or mystery.

Nay, even they themselves do not judge one another. Thus Dr. Foster, when asked whether "a great many" of

Schiff's experiments were "not needless," and "not to say wanton, but experiments which" Dr. Foster "would be sorry to conduct" himself, answered: "It is very difficult to say when an experiment is needless. One feels a difficulty in sitting in judgment on one's fellow-men in that way" (2405).

But all this delicacy of judging, and all the contempt for "lay inspection," and doubt of the public capacity to judge even of the *results* of experimentation, is thrown aside when some assertion can be made, of the truth of which few can judge, but which, if "established," is a "*convenient doctrine*," as the Commissioners describe some of the "physiological" lucubrations.

(2.) Secondly, those who made, as far as they *did* make, the discoveries claimed to the credit of the new school, were not mere "professional discoverers," but in general, medical men, concerned about some *particular and definite subject* bearing on their profession, and directly related to human benefit; and this accounted for their attention to the subject, and for the *practical* character of their discoveries. Such alleged discoveries are therefore foreign to the exclusive experimenting class and system.

(3.) If Newton *did* discover the law of gravitation by the fall of an apple, or if Galvani discovered galvanism by the accident of a frog jumping on his plate, through getting a spark of electricity, if any mechanician discovered the use of steam by seeing it lift a kettle lid, it would *not follow* that a special class of men should *devote their* lives *to shaking apple trees, playing with frogs*, or *sitting*, poker in hand, to *keep a kettle boiling*, on the ground that *nobody knows what may turn up;* since Galvani's frogs, or wife, or he himself, discovered what has ended in telegraphs and other modern improvements; all from the fact of those frogs being, through his wife's delicate health, brought on to the premises to play so prominent a part in the history of discovery.

Hence we are told that licensing some, and so excluding others—which is the present system—might have prevented Galvani's grand discoveries, or might, as some witnesses supplicatingly suggested, have prevented Harvey achieving his discoveries.

The case of Galvani is put, by one witness, in a way *not flattering* either to physiological discoverers or to woman's intellect. Dr. M'Donnell who, as an Irishman, should have had more chivalry, and, as an investigator, more pride in his "craft," tells us:—"We never know how great the result may be." "When Galvani first saw the frog's legs jumping by electricity, he never knew that the result would be to bind the whole world together by the electric telegraph" (4575). Here, as in other instances of galvanised admiration, no credit is given to the inventor of the telegraph; it is all put down to the frogs, and afterwards to Mrs. Galvani, as a proof of *how little intellect is required for a discovery!* The words are:—"There is no such thing as a gift of discovery; it is a patient, painstaking carefulness, and a *desire to record certain facts truthfully;* and a *very humble man,* of *limited intelligence,* may possess *that. In fact,* I *think* it WAS GALVANI'S WIFE who *saw the frog jump upon the* plate, and MADE THE DISCOVERY that has done everything for us in electricity. She was a delicate lady, and *was ordered frogs;* they were on a plate in the laboratory, and she drew her husband's attention to *the fact* that a spark from the machine made them jump" (4580).

All this is scarcely a reason for every wife being "delicate," and being "ordered frogs," who may "jump to a conclusion," as Mr. Huxley says Claude Bernard, the great investigator, "jumped to the conclusion" that *curari* is not an anæsthetic (4773).

Now, though some navigator discovered the continent of America, this is no reason why the best ships, and the *elite* of the officers and men of Her Majesty's navy, should be for ever occupied in exploring to find more continents, and give,

as a reason in defence of their employment, that it *was only by sailing that America was discovered.* If they sail for years and find nothing, but only hope some day to do so, and then declare that meantime they are "getting more precise knowledge," though not precisely a knowledge of more continents, it would scarcely be a defence of the waste of their own time and of the public money.

(4.) If some great calamity, as a war, or some crime or treachery, as selling Joseph into Egypt, was shown to produce beneficial results, it would be no great reason for a profession to be established for the purpose of fomenting wars, or selling brethren into slavery, even if the profession pleaded the great advantages or discoveries which had resulted from the process. And so, if some benefits, however inappreciable, have resulted to man from past occasional torture of animals, it does not follow that a professior, specially devoted to this kind of experiment, should be supported or permitted by the public.

(5.) But *if, on the contrary, the benefits are mythical,* and the *discoveries* themselves, *so far as connected with painful experiments,* are *doubtful,* even the excuse of utility is destroyed. That these discoveries and consequent benefits, as results of experiments, *are* doubtful, and that they are certainly *not* the results of exclusive devotion to experimenting as a distinct profession, and therefore ought not to be advanced in defence of *the system,* is proved by the three canons of historical and scientific criticism, advanced by two "experts" and a "lay" member of the Royal Commission, which we next adduce, as *the utter and confessed failure of this retreat into history,* to prove the value of the system whose actual and *present performances* should constitute its claim to continuance.

1. The first canon, requiring clear ideas expressed in definite words, was earnestly insisted upon by Dr. Burdon-

Sanderson, in his evidence before the Royal Commissioners (2314).

2. The second point—honesty and frankness of statement, as not passing off as recognised truth, any opinion respecting which contrary views are maintained by fairly competent advocates (4172-3-4)—is laid down with some earnestness in three questions by Professor Huxley, and though the caution was not at all necessary in the case to which he applied it, there can be no doubt that the rule is a good one.

3. The third canon of criticism is a combination and practical application of the other two, namely, the duty of not using on our side, nor accepting from the opposite side as "facts," what are not clearly such; but are as much themselves properly in question as the points which they are advanced to prove.

This important rule, that the facts should be "indisputable," was indicated by Lord Winmarleigh, in reference to a whole series of "*facts*" poured out upon the Commissioners, in which Jenner's vaccination discovery (3027) was based on these physiological "experiments on living animals!" (3063). This is a specimen of those "facts" which "bristle around us everywhere" (5525), but which fade into fictions on "investigation."

The application of these three rules to professed "discoveries" and to the "missing links" between such alleged discoveries and benefits derived from them, is important.

(1.) The rule for exactness of thought and language, as given by Dr. Burdon-Sanderson, together with the illustration with which he accompanied it—viz., the case of "new discovery"—tells fatally against the *veiled* language in which the purposes, prophesies, or promises of experimenters are often wrapped up; but it more *directly and unequivocally undermines and destroys* the SPECIFIC CLAIM TO DISCOVERIES, which is the main historical defence of

experimentation. For, according to his more exact method of thought and speech, *there is no such thing* as a "new discovery;" and, of course, an *old* "discovery" could bring no credit to any "investigator."

According to Dr. Burdon-Sanderson, nothing is more "vague," in opinion and in word, than the question of "discovery." So thoroughly is he convinced of this, that he would *eliminate the word "discovery,"* and adopt the term "investigation," or "*scientific* investigation," though he does not indicate the difference between "scientific investigation" and "investigation" pure and simple. At all events, he objects to the phrase, "a new scientific discovery," as without definite meaning, and as having no proper application; for, as he declares, *nobody can tell what a new discovery is,* or who made one.

This surrenders a great deal, that at other times is insisted upon, as the glory and defence of experimentation.

Thus, when asked his opinion respecting a proposed Bill regulating "new scientific discovery," he replied: "2314. A 'new scientific discovery' *has no definite meaning* in relation to research . . . therefore, what is really required there [as the terms for such a Bill] is something of this sort: 'Any person for the purpose of investigation, or something equivalent to investigation, or scientific investigation,' it does not matter what expression is used, but it must be something to which one can attach a *bona fide* meaning; I mean one that one has not [as in the phrase 'new scientific discovery'] to take a great deal of trouble to assign a *meaning to.*

The question—WHAT A NEW DISCOVERY IS, *or what it is not,* is a question that *might be discussed endlessly.* One man might say, 'This was *anticipated* a hundred years ago' [as is said of the discovery of the circulation by Harvey], and you would find just as many people who would say, 'It was not known at all; we were *absolutely* ignorant of it before,' because *one* would assume a mere

vague expression of a thing to be an *adequate* statement of it, whereas the *other* would expect an *exact* statement of it and in this way there would be a constant conflict of *opinion* between people who spoke exactly and people who spoke vaguely."

There is some confusion here, as to "vaguely" and "exactly;" for those who say, "We were *absolutely ignorant of it* before"—just because, though "anticipated," there was not "an *adequate* statement of it," but a "mere vague expression of it"—must surely not be amongst the "people who spoke exactly." But this is implied.

However inexact Dr. Sanderson was in this application of his distinction about speaking "vaguely" and speaking "exactly," there is at least no doubt, whether he regards "*discovery*" as too "vague" a term for *use* where precision is necessary; and that the question of what is tautologically called "a new discovery," is open to be "discussed endlessly."

What, then, becomes of all the boasted "discoveries" set down as the achievements of great genius, working in the experimenting way, where, as is often assumed, eyesight is the only great capacity required?

Here is to be noted that great law—the gradual growth of thought—according to which, as Professor Tyndall, in his oration at Birmingham, taught us, there are no more leaps and breaks in the progress of human intelligence than the molecular philosophers acknowledge in the course of Nature.

However true this molecular speculation may be, there is some truth in *the gradual growth of thought and science;* so that each enquirer, coming with the advantage of the accumulated observations and reasonings of his predecessors and contemporaries, has a chance, like Harvey, to take one step beyond his teachers, and see a truth more fully, which they and others saw only as an adumbration, or, as Dr. Gull termed it, "an anticipation" (5539).

Instead, therefore, of hopelessly leaving for endless dis-

cussion the difference of opinion between those who say of a "discovery"—"it was anticipated"—because some "mere vague expression of" it had long foreshadowed and pointed the direction of fruitful observation—and between those who rashly say "we were absolutely ignorant of it before," we should rather investigate, clearly and fearlessly, the progressive stages of knowledge, and give to each promoter of it his due mead of praise.

We must not, for the sake of any "convenient doctrines," as the Commissioners not obscurely hint against the exact physiologists (Report xix.), neglect "research" into the true history of the gradual advancement of knowledge, which may afford hints for its future progress by the same methods of patient thought and careful observation.

But if, discarding this line of investigation, physiologists justify their indifference to it by the difficulty of forming an opinion "*what* a discovery *is*," they are *not therefore at liberty to claim the glory of all discoveries of any value* as belonging to their peculiar practice of experimentation; but, on the contrary, they render all such claims absurd or impossible.

Dr. Sharpey, for thirty years Professor of Physiology in University College (386), gave the same testimony as Dr. Sanderson as to the *impossibility of defining a discovery*; and argued that if the Legislature passed a Bill permitting only those experiments which were "for discovery," it would be impossible to obtain a verdict against any one for infringing the Act, because nobody could decide *what a discovery was* (453). Therefore he, like Dr. Sanderson, who was at the head of the Government working experimenters, declined the word "discovery," and proposed instead, as the "purpose" of the experiments—"for scientific enquiry."

This would cover anything, except "discovery," which they *claim* as a defence of their method, and *deny* as an undefined word, so "vaguely" used as to have no specific meaning.

Dr. Sharpey said: "As to the question of the purpose of them [the legalised experiments], the Legislature will probably find it difficult to define what *are* experiments 'for the purpose of *discovery*.' It is *perfectly well known*, in the history of science, that there has been *no more fruitful cause of dispute* than the question, *What is a new discovery*, or an original observation? and if the case came ever into a court, I have no doubt that you would get a number of eminent persons on one side, and an equal number on the other, who would maintain different opinions upon that, and give different evidence, though both sides would be perfectly honest in their expressions of opinion; and therefore I believe it would be impossible to get a conviction for the infringement of such an enactment" (453). Meaning an enactment forbidding any experiments except for "discovery," and this *solely because*, according to this school of physiology, *nobody can say exactly what* IS *a "discovery."*

(2.) Not only does Dr. Burdon-Sanderson's rule and illustration *exclude discoveries* from the achievements of the experimenting school, but Professor Huxley's canon makes it *dishonest* to adduce such discoveries as those of Harvey, and Bell, and Hunter, to build up or to defend the experimenting cause. For Dr. Sharpey declares there are competent men on both sides to say of any case, that it *is* or is *not* "a discovery;" and Professor Huxley earnestly enforced the fact, that it was contrary to "the interests of common justice" to state a case as "a fact," when there are "*two views* of a case current on good authority." But we find "discoveries" by Harvey and others, from vivisection and other "experiments on living animals," constantly affirmed as absolute "facts;" although, first, the party in whose interest these assertions are made, affirms that nobody knows what a discovery is; and, secondly, it is well known, or what is nearly the same thing, ought to be known, by

those who unreservedly attribute such discoveries to vivisection, that others maintain the contrary, and give arguments in favour of their opinions.

Mr. Huxley, as one of the Commissioners, examining a witness, who spoke of Paul Bert's "curarised dog" as *suffering*, because *only under curari*—which had not then been, and is not yet, proved to be an anæsthetic—took the witness to task for not having referred to the "convenient" physiological doubt as to the anæsthetic property of curari.

Mr. Huxley said: "You did *not think it necessary to state* here, *in view of the popular feeling* on the matter, that although this [non-anæsthetic character of curari] was the view of M. Claude Bernard and other persons *at that time*, yet that *of late, whether rightly or wrongly, other* competent persons *had argued* that that was not the case?" (4172). He meant that Dr. Hoggan, the witness examined, should not have said positively that the dog was "suffering," because some had raised a doubt—"whether rightly or wrongly"—respecting the effects of curari. He was to give the experimenter, not the victim, the "benefit of the doubt."

Mr. Huxley puts it more strongly in the next speech-question, wherein he argued against the witness: "4173. Is it not rather a serious thing when you are *dealing with persons who do not know anything about a matter*, and who have no access to the channels of information, when you know there are *two views of a case current*, on good authority, *to put only one* before them?" It is more vehemently put in question 4174: "My question was whether," "in the interests of commen justice, it is not well *to state a disputed case* in such a manner as shall *not lead* people to believe that it is *not* disputed?"

This rule SETS ASIDE ALL that is said about Harvey, Bell, Hunter, and others, as having, by painful experiments on animals, obtained their discoveries.

In the *pattern case* of Harvey, for instance, how many

people can say: 1st, what he *did* discover; 2nd, *how* he did discover it—by what particular experiment, or by any experiment at all; 3rd, whether the experiments *verified* the inference, or *led to* the inference? Any one reading only B. W. Richardson's article on Harvey, in the *Gentleman's Magazine* for April, 1878, would hardly know whether Harvey discovered *anything*. Persons of only general information on the subject would know that he did *not* discover "the circulation of the blood," but *contributed towards an explanation of it*, by his observations respecting the *movements of the heart* in relation thereto. It may be well here to note, that it is not the machinery in motion that explains the mechanism, but *the mechanism that explains the motion*; and Harvey's main thoughts turned on *the mechanism* of the heart—the valves, which gave direction to the current, gave direction to his enquiries and conclusions.

It is enough, however, for the present purpose *to clear the principle of the argument*, which, without the details of the history of Harvey, Bell, Hunter, or others, SETS ASIDE THE CLAIM of experimenting physiology to the credit of any of the discoveries made by these eminent men. For, first, according to Dr. Burdon-Sanderson and others, nobody knows what a discovery is, or who made it; and secondly, according to Professor Huxley, it is contrary to "the interests of common justice" to affirm as a truth what, "whether rightly or wrongly," is disputed; and thirdly, according to Lord Winmarleigh, the "facts" adduced in favour of any cause should themselves be "indisputable:" and *all these rules* are *violated* by *every* claim to discovery by experiments on living animals, as attained by *former* experimenters. We may add that, if the *present* new system of professional exclusive engagement in exploration, were *worth its salt*, it would *defend itself* BY ITS OWN WORK. *What* it *has done for man or for animals* is our next enquiry.

CHAPTER IX.

PUBLIC EXAMINATION OF THE NEW SCHOOL, AS TO THE VALUE OF ONE DEPARTMENT OF ITS WORK, WHICH IS A SPECIMEN OF THE REST, AND A PROOF OF ITS FAILURE TO DO GOOD TO MAN OR BEAST.

HAVING examined the claims of experimenting physiology on scientific and logical grounds, as a professed method of securing certainty and advance for the healing art, and shown how this fails intellectually; and having considered its claims on the ground of results from former discoverers, and shown this fails historically, both from the denial of any sure mark of *what is a discovery*, and from the *uncertainty of the "facts"* relied upon; we may next consider its claims, on account of *its own work* in providing suggestions and means for improving the health of the public. True, we have learned that this is "*not the business of these men*" as physiologists; yet, since it is what they undertake, and in some cases are employed for, and as it is the *only valid plea for their profession* or employment, we shall inquire *what they have done* for man or animal.

I. *What it has done for Man in the case of Cholera.*

Mr. John Simon, the Medical Officer of the Privy Council and the Local Government Board, and under whom Dr. Burdon-Sanderson, Dr. Klein, and others, were at work in experiments for the national health, when asked:—"1406 (Mr. Forster), What *advantage* do you think has been obtained *for sanitary purposes* by any of these experiments?" replied: "First, as regards the cholera experiments, ALL *the advice that I give* to the public about cholera refers to the *infectiveness of the discharges*." This implies that

people generally regarded "the discharges" as not objectionable elements of diet; and that medical practitioners, who are to be guided by the "lamp" held out by the "professed physiologists," also were either favourable to the mixture, or indifferent as to its results. His added statement, that "the infectiveness of the discharges can *hardly* be said to have been *directly tried* on any but the *lower* animals," is, we hope, quite true: though we should scarcely imagine that even the experiments on "the lower animals" are necessary to induce the higher ones to become "total abstainers" from "choleraic discharges." The pretence is, to give a *scientific reason*, so that even our food and drink shall not be taken "empirically;" "and," says Mr. Simon, "*all* the advice that I give to the public about cholera, refers to the infectiveness of the discharges."

When asked (1407) "in *what way*" he had "obtained information" "really useful for sanitary purposes," he said: "By improved knowledge of the causes of disease;" but this professed "improved knowledge" was itself the "information" and not the "way" in which that "information," such as it is, had been "*obtained*."

The "improved knowledge" was (1408), "that if the discharges" "from cholera patients" "get to other people, those other people will, in all probability, have the cholera." Whether this "knowledge" was needed, whether, "in all probability," people would not prefer "to avoid" such ingredients, even if not considered "infective," is scarcely open to doubt.

How the "information" that it *is* "infective" was professedly "obtained," we thus learn:—"1409. And *that* you have *found out* (!) by actually testing it upon animals? *That* is my *belief*." But even this "belief" is very faint, and he "prefers to rest upon the *general principle*"—"the unanimous belief of followers of medicine"—that "a more exact knowledge of the causes of disease" may aid in preventing it. So far, therefore, as the "general principle"

is concerned, physiology need not hold out "a lamp" to the benighted medical practitioner. Nor were "experiments" needed for this particular case, to prove that "choleraic discharges" are ineligible ingredients in food or drink. To the enquiry, whether the experiments "rather" increased "physiological knowledge than" tending to "remedy a special evil?" (1410) he replied, by indirectly confessing the fact:—"I put the justification [*i.e.*, of experimenting] on general grounds." In other words, they had found no "remedy" for "a special evil." The further statement that:—"In cholera, we *believe* that experiment has furnished an exact *rational basis* for the *prevention* of the disease," only shows how easily experimenters are deceived. It implies that the *origin* of cholera is in imbibing "infective" discharges from those who already suffer from the disease. That one particular way of *spreading* the disease, may be avoided by defending drinking water from that source of pollution, is as true as it is obvious; but it is not true that "experiments" on animals had "found" this "out;" and if it were true, it would not follow that this precaution against one very limited way of *spreading* cholera, "furnished an exact rational *basis* for the *prevention* of the disease." We might as well say that the invention of umbrellas "furnished an exact rational basis for the prevention of" rain.

The phrase, "exact rational basis," is a pedantical way of assuming something "scientific" in experimenting guesses, as opposed to the "empirical" knowledge deduced from "observation" of circumstances actually occurring in ordinary human experience.

The fact is, that "observation" discovered this source of spreading "cholera;" and then, needless experiments were made to confirm what needed no confirmation.

The efforts made to conceal the fact that observation had proved, what experiments were said to discover, were all in vain.

Mr. Forster naturally said:—"1411. Then I think I must

ask you AGAIN, in WHAT WAY have these *experiments* enabled you to *give advice* to the Government or to the public to *avoid* the cholera, *or* to check its spread?" Answer—"In that case thoroughly." This does not tell us "in *what way*;" but it assumes as to what *extent*—"in that case *thoroughly*." In *which* case?—"the public to avoid cholera" altogether, or "to check its spread?" "In what way" is it done "thoroughly" for either purpose? The question was therefore obliged to be put still "again":—"1412. Will you tell us HOW they [experiments] have done so?" This *should* have brought the answer, but it secured only this disappointing reply:—"By *showing* (!) us, for instance, with what extreme watchfulness we must *keep even the slightest taint* of choleraic excrement *out of drinking* water." And is *this* all? Were experiments on animals, with this "infective" agent, necessary to learn a matter of common cleanliness?

It reads like the cross-examination of a pretender:—"1413. You administered very small doses of choleraic discharges to living animals" and thus "*discovered* that the disease was communicated to the animals?—Yes." "1414. And *from thence* you have *drawn the inference*, that it is of the utmost importance to the health of the public, to prevent *even very small doses* of that discharge from being communicated to the human frame?—QUITE SO!" As if the "inference" had not been drawn from "observation," and as if it needed much "drawing" at all.

This "original discovery" is followed by the helpless confession:—"And ALL our *practical* advice to the public as to *how* they shall deal with individual *cases* of cholera *when* they occur, *and* the *precautions* they shall take against the *spreading* of the disease, *refers to that* experimental basis."

Now this does *not* show "how to deal with individual cases of cholera when they occur;" it only shows how to avoid *one way* of "spreading" it; and this was *known before*, without experiments, namely, as Mr. Forster put it (1415), "from observation." When this fact was put to Mr. Simon,

whether "observation" had not already taught what the experimenters professed to discover, he answered:—"I can hardly say that." No, but he could not deny it; and goes on to admit it:—"I did not consider it to be *proved*. It had been SUSPECTED."

This is the cant term by which knowledge from "observation" is distinguished from pretended discovery by "experiment." He admits that "it was *a theory* of cholera" taught by Dr. John Snow in 1849; but this, which everybody knew before, and which anybody might have "suspected," was "*converted* into *such* a degree of *certainty*, as can *properly* justify advice to the Government Department"—viz., to discourage the consumption of "an infinitesimal portion of choleraic discharges," as a part of diet!

This is the "infinitesimal" comfort for the admirers of experimental physiology.

These experimenters look at one of the scourges of mankind, and undertake to avert it by *a special method* of their own, in one particular or possible point of danger, and they end by *confirming* or "verifying" what was "suspected" and published twenty-six years before, by Dr. John Snow, but which is depreciated under the title of "a theory," in order that any credit belonging to it may be transferred to the second-hand experiments of "original discoverers."

II. *Benefits to Man in the case of Consumption.*

Physiology, as peculiar to the "professed" "experts," is greatly magnified by a frequent reference to the numbers who die of consumption, and by many vague speculations as its "cause" and "nature."

Mr. Forster inquired (1420): "Now, as regards *consumption*, do you *consider* that there has been any *practical* advantage in your experiments?" Instead of saying plainly, "No," the witness replied: "*We* have gained very great advance in knowledge." Do you mean knowledge as to how to cure consumption? Well we must have faith, that some

day, even *that* may be learned; or, as the witness said, "I *again* rest on the general position" "that, *with* such advance in knowledge, we shall *necessarily* get results in practice."

What "knowledge" they had "gained" turns out to be what everybody else knew without experimenting on animals; but at present the physiological "knowledge" is barren—no medicinal fruits have grown on this "tree of knowledge, of good and evil."

Mr. Forster kindly admits (1421) that "knowledge" is very good, but suggests that "it would be very important to know that PRACTICAL RESULTS have been obtained. *Has* any practical result been obtained with regard to consumption?" To this, which was "the second time of asking," came the very discouraging answer: "*We* have gained (!) the knowledge of the communicability of the disease from subject to subject." Even this was indefinite (1422): "Do you mean by contagion?—By certain ways of contagion; and *that* will, of course, have a *very great influence* in the advice to be given in reference to the disease; but to speak of *that* as an *immediately practical* result, is, perhaps, *not* putting it *quite* correctly." In other words, it would not be true; and this mysterious promise of "very great influence in the advice to be given," collapses.

The "very great advance in knowledge" is a will o' the wisp. There is neither "science," nor "practical advantage," after all this parade of progress.

We have reached no "benefit" yet, in this examination. Even the "improved knowledge" claimed to the credit of "experimenting" is generally confessed to be "obtained" without experiments.

"What we know of it [tubercular disease in sheep] is that it is inoculable from subject to subject;" and any keeper of sheep may "observe it" (1423).

"1424. Would what you have *arrived at* ["communicability" and "inoculable"] induce you to form an opinion"

"as to the danger of a healthy person being in close communication with a consumptive person? It would *undoubtedly* do so."

This rich pail of milk is immediately kicked over and spilled, leaving nothing to the credit of experimenting discoveries, as being altogether foreign to that method. For we are told of Dr. Herman Weber's statement to the Clinical Society, of *facts observed* respecting "tubercular infection" "communicated to women through their conjugal relations with men who have pulmonary pthisis in a quiescent or very chronic state."

What these observations on men, have to do with experiments on animals, needs to be explained. But this is what physiologists "have arrived at;" and by which they "would undoubtedly" be able "to form an opinion" about the "communicability" of consumption; if they had never made nor heard of "experiments."

This confession and illustration, that all they know is from observation and not from experiment, is well followed up by the assertion that:—"Experimental inquiry has *opened* an *immense field*, a field of which it is *impossible to see* the *eventual* outcome, in the direction of preventive medicine." Nothing has come out of it yet; and all this present science is a large draft on our faith as to the future.

If by "experimental inquiry" were meant, not inquiry by experiments on animals, but "experimental" knowledge, from observation of the "experiences" of mankind; and of what presents itself in the ordinary course of nature, in the animal kingdom, we should then have an "immense field" fertile in useful products, instead of the barren ground of speculation, whose past and present "outcome" is, to steal the results of "observation;" and "whose [own] eventual outcome" "it is impossible to see," because there is nothing yet—to look at.

III. *Benefits to Man in cases of Fever.*

This is another "immense field" of experiment, promise, and failure.

The inquiry here is very short, and refers only to scarlet fever.

"1425. In investigations with regard to scarlet fever" (1426) "are you making experiments upon animals?"—"No."

Then these "investigations" cannot be "*experimental*" in the sense of "practical physiology," or pathology, and consequently are outside the special field of "original research."

When asked (1427), "*Why*" "not experiment upon animals for *this?*" the answer was: "We have no reason to believe that scarlet fever is communicable to them."

This is a suspicious answer; for, according to "all the analogy of science"—"the general grounds," before resorted to—most other human diseases can be communicated to animals, and it is *solely* by *this* "communicability," and the study of diseases thus produced in animals, that the third (class *c*) sort of experiments are to benefit mankind.

Here we come to a baulk, in the case of scarlet fever. It is significant that we are not told whether the experimenters *had tried* and failed to communicate this disease to animals.

But, one would say, if they cannot "experiment" on animals, in this case, and so lose *their only instrument of discovery*, then they give it up; but whether they do or not is carefully concealed, as when asked (1428): "Does the fact that you are not able to make such experiments *impede you* in the investigation?" the witness does not say, but only observes: "It *might* be a great advantage to us, in the study of the disease, if we *could* communicate it to the *lower* animals, so as to watch its earlier processes."

We are not told what method of study they adopt in the impossibility of experimenting on the "lower animals."

There is not even a "very great advance in knowledge" in this case, from the method which is inapplicable to it. Mr. Forster, who has already confessed that "knowledge" is very well in its way, now again presses the practical inquiry: "1429. Your object in making these investigations" "is not merely to acquire *knowledge*, but with the *hope* of acquiring SUCH *knowledge*" as would be *beneficial*, and "would *enable* you, as their *medical* officer, to advise Government, how to prevent the spread of such a disease as scarlet fever?—CERTAINLY." Now this is "certainly" *not* done, as the result of any experiments referred to in this examination.

The "certainly," above quoted, is followed by the "opinion" "that *scientific* advice concerning prevention must, in all cases, refer to exact knowledge of causes."

Here it is prevention and not cure that is referred to, and no reason is given why the "knowledge of causes," is more open to *experimenters* than to *observers*. In fact, the experimenters must, in general, take their idea of "causes" from *observation*, before they begin their experiments; and they *learn*, "by experiment," that what had been clear to "observation," is really true; for instance, that Dr. Snow was right in "suspecting" and publishing the "theory" that "choleraic discharges" are "causes" or agents for infecting with cholera.

They try what has been seen; and call trial and proof of known facts the result of "original research."

But what "practical results" are secured from this special line of investigation? "very great advance in knowledge;" or speculation.

But where are we favoured with "*such* knowledge as would enable" "their medical officer to advise the Government" how to "prevent the spread," or to provide for the cure, of disease?

We have "*found out*" (?) that "choleraic excrements" are objectionable, that we must "keep even the slightest taint

of it" "out of our drinking water;" whether the same was not learned "from observation," I can hardly say: "(1415) We have learned the "communicability" of consumption from cases of men, without "experiment" on animals, and we know that it is "inoculable," and—this is all.

Now, if some *remedy* had been discovered for the cholera where it had seized on patients; or some method of *preventing it from visiting us* at all; and if, instead of the second-hand information as to the "*communicability*" of consumption, we had some original "discovery" and "verification" of the CURABILITY of consumption, this third line of inquiry by infecting animals with diseases in order to find in them a cure for the same diseases in man, would not, on its trial before the Commissioners, have failed so ignominiously.

IV. *Benefits to Animals, and, through their cure and preservation, to Man, in the way of increased sustenance.*

In one part of the Royal Commissioners' Report it is stated that animals participate, as well as man, in the beneficent results of experimenting physiology. It is but fair they should; and perhaps they *do, i.e.*, in the sense that man gets nothing; for in this respect animals are participators, "as well as man," but no better, though they "pay the smart."

It is of great public importance that cattle plague, sheep-pox, and other ravaging diseases in animals, should, if possible, be conquered; but here no more help is furnished than against cholera, snake-bite, scarlet fever, and consumption amongst mankind.

When the physiological experimenters are put upon their trial, and the *practical* test of their "practical" science is fairly applied to the case of animal diseases, the answer is a confession and an apology, and what Sir William Gull calls "not a joke" but "an anticipation" (5539). It is all "anticipation;" there is no realisation.

When asked in plain English (1417)—"WHAT CASES can

you give" of "prevention," apart from cure, "of disease in the lower animals," from your experiments? Mr. Simon said:—"I have very great difficulty, insuperable difficulty, in explaining the position *clearly* in *popular* form, and that is why I preferred to rest upon *general scientific* principle."

This was a scientific way of saying that they "can give us" *no* "cases" of any relief to the animal creation, even by way of "prevention;" and the sooner this is put "in popular form," the sooner shall we secure the "prevention" of that torture to "the lower animals" which neither relieves them, nor the higher animals who inflict it. The further apology, that "we (physiologists) are going through successive stages of endeavour" "trying" "to amend our medical knowledge in accordance with the rules which have been discovered to work well for all *other branches* of knowledge;" and "trying to make our knowledge quite precise," is simply cloaking practical failure under pedantical nomenclature.

The plea that they are "at present in but an early stage" of "endeavour," contrasts strangely with the frequent boasts of the immense strides lately made in medical practice by this kind of physiological "research."

When we come to the special case of practical benefit, the defence again flounders in this deplorable fashion:—

"In regard to sheep-pox we have *been getting*, as you will see in that blue book, *exact knowledge of the contagium* of the disease."

Now, "the contagium of a disease" is the specific poison, or infecting influence which produces it; but the experimenter, as before remarked, *has this agent in hand*, or that which contains the germs, when he *begins* to produce the disease, in order to study it, in an animal; as, for instance, the famous and fatal "pyæmic fluids" used by Dr. Burdon-Sanderson for producing certain morbid conditions. *There* is the *contagium* to begin with—it is the agent by which the operator performs his experiments.

But if by having "*been getting* a knowledge of the *con-*

tagium of the disease"—sheep-pox—we are to understand that they have actually "*got*" that knowledge,—some really more intimate acquaintance with the structure or nature of the *contagium*,—then it would be easy for them to give that knowledge to the world.

But when this is done, if it ever *is* done, the next step would be to tell us *how to conquer* that *contagium*.

They DO NEITHER. We are brought up to the supreme moment of expectation—"have *been getting*" "exact knowledge of the *contagium*"—and then, explain how to deal with it?—No! The next step is to say—we are as helpless as ever on the subject; *our* knowledge is "*not*" "power."

Accordingly, after exciting our hopes in "getting knowledge"—which is the speciality of the system—we land on this fog-bank:—"Hitherto, I fully admit, we *cannot* prevent sheep-pox *otherwise* than by the *quarantine arrangements* which we *could* have practised BEFORE those experiments were performed."

Risum teneatis amici? Who can help laughing at such grave simplicity of helpless confession after such high sounding professions?

This confession, "generally so good for the soul," immediately loses its virtue by the *contagium* of that illusory hope which inveigles men on, till it turns distrust and despair, from practical disappointments, into a feverish desperateness; like that of Little Nell's grandfather, who, having always lost in play, felt sure that his luck would turn if he could but find stakes to renew the game.

It is to this hopeless pertinacity and despairing belief that the next sentence, immediately following the above confession, is appropriately addressed.

For after saying that after all these experiments—"*getting exact* knowledge of the contagium of the disease"—"we *cannot prevent*" that disease, "*otherwise* than by arrangements which we *could* have practised *before* those

experiments were performed," instead of *giving it up* the advocate goes on to say:—"But, as I said, we are going through *progressive* work, which has *many stages*, and are NOW *getting* MORE *precise* knowledge of the *contagium*."

These terms are as good as the "sufflamnigations" and other words "of art" used by Dousterswivel in the Antiquary when he "discovered" the "treasure" in Mistigott's tomb, by *his* "experimenting" process.

The "stages" in this "progressive work" are not very clearly marked, and seem rather theatrical "stages,"—more for show and scientific amusement than for real service in the serious business of life.

Before, we had been "getting *exact* knowledge," "quite precise;" we are "*now* getting MORE *precise* knowledge of the *contagium*."

This is the perpetual attitude of this *promising* science, it is on the verge of an idea; and has nearly caught it; whether, when found, it will be like that "lower animal" which had only two faults—"difficult to catch, and of no use when caught"—is not said.

How near they were then to catching this idea, getting a firm hold on the *contagium*, if not on its therapeutic antagonist, is seen in the continuation of the defence:—

"By these experiments on sheep, it has been made *quite* clear that *contagium* of sheep-pox is *something of which the habits can be studied*, as the habits of a fern, or a moss, can be studied; and we *look forward* to opportunities of *thus studying* the *contagium* outside the body, which it infects." The rule, "First catch your hare," is applicable here. We know "ferns and mosses" as undoubted organisations, but this "*contagium*"—as some mysterious product, process, or element of disease—eludes our search; we cannot "study its habits," till we have "more precise," or at least "exact knowledge of" it.

Can you get it "outside the body which it infects," and place it so under observation that we may have oppor-

tunities of thus "studying" it? To such a question Mr. Simon modestly replies: "This is not a thing to be *done in a day*, nor, perhaps, in ten years, but must *extend* over a long period of time" (1417).

But if the *contagium* is discovered, and if "it has been made quite clear" that "its habits can be studied [if not reformed], as the habits of a fern or a moss can be studied," why should we wait for "ten years" even, much less "a longer period of time," before learning *what* it is, and having it placed before us for "study?" It has been "traced right to the end of its course in the infected body," and is *therefore* (?) to be hereafter traced "outside" the infected body. Such is the hope with which this *discovery* of the weakness of the so-called practical physiological research, crowns the edifice of promise.

Accordingly, Mr. Simon concludes his explanation of benefits to animals, by learning the *contagium* of sheep-pox, with these words:—

"Dr. Klein's present paper represents one very important *stage* of a *vast special* study. He *gives* the *identification* of the *contagium* AS SOMETHING which he has studied *to the end*, in the infected body, and which can NOW, in a *future stage*, be studied *outside* the body" (1417).

Here we have two "stages," but have reached only one of them; the first stage is, tracing the *contagium to the end in* the body, the second "stage," "now, in the future," is "*outside* the body." Could anything be more indefinite and delusive? Is this the science that is to "make our knowledge *quite* precise?" Is there any precision in the language — is there any *real idea* under the language? What is meant "by the IDENTIFICATION of the *contagium*, as *something* which he has studied *to the end*"—and what "end?" By *what processes of observation* was this "identification of the *contagium*" traced through its stages, and *where* did it "end?" Can one trace "the identification of the *contagium*" of fever, as *some reality in the body, to its*

end, and *then fix it* as an "identification" "outside the body," and "study its habits?"

After amusing or confusing the examiners with these *speculations* about practical experiments, worthy of the inquirers after "the philosopher's stone," they drop off suddenly from *molecular metaphysics* to a plain confession that *this* is *all* they have to offer—for the present.

Accordingly, when Mr. Forster, breaking through this tangled cobweb of indefinite verbiage, asks again a plain practical question, one monosyllable sums up the value of these performances:—

"1418. But, *up to this time*, have ANY of your experiments *led to a result* which would enable a veterinary surgeon *to treat* sheep-pox *with more hope* of cure than he did before?—No."

Here we come to the point, and the truth stands in its naked unsophisticated condition. To cover this retreat and failure, Mr. Forster draws on the future, and kindly says:—

"1419. But you *hope* that they *will* do so?—Yes, or rather with (especially) *more hope* of prevention."

So he *gives up cure;* and is faint as to "prevention," which does not go beyond the old "quarantine arrangements."

Here it is to be specially noted that even the problematical discovery of the *contagium* is not regarded as an aid in therapeutics; all that is dreamed of is a new way of prevention; which, in a further stage, may "result" from experiments that have hitherto failed. So we trace "to the end," in the experimenting system, the *contagium* of delusive expectations and promises, and "now, in the future stage," can study it "outside the body" "as something" which hides itself in words without ideas, and benefits man and animals more in promises than in performances.

CHAPTER X.

THE ANÆSTHESIA* APOLOGY; OR, THE PROFESSED ABOLITION OF PAIN AS NO LONGER, OR ONLY INFINITESIMALLY, INVOLVED IN EXPERIMENTS.

I. *This apology not really believed in by the Commissioners, who seemed to adopt, if not to suggest it.*

The pity and indignation awakened in the public mind by the revelation of certain experiments abroad, by the "suspicion" that similar ones were performed in England, and by the directions for "Beginners" in the Handbook of Dr. Burdon-Sanderson and others, occasioned the Royal Commission of Inquiry (1866–7) into the extent to which "experiments on living animals" were conducted, whether there were any abuses, and how such abuses might be corrected.

The delusive palliative of anæsthetics was seized upon to lull the public mind, under the pretext that the so-called physiological experiments could now at least be generally painless.

It was admitted that the leading boasted discoveries attributed to the experimenting method—as the circulation of the blood, the lacteal system, and the functions of the nerves—"which lie at the very foundations of our present physiological knowledge," were all made before anæsthetics were employed or known.

It is acknowledged that experiments in these directions involved the infliction of great agony; and that "if this inquiry had been instituted at some former time," namely,

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* "Anæsthesia" is Greek for Insensibility. "Anæsthetics" are the agents producing insensibility.

before anæsthetics were introduced, it would have been necessary to weigh in the *one* scale the infliction of *great and, perhaps, protracted* SUFFERING, in the *other*"—"THE KNOWLEDGE, most important to mankind, but *only to be looked for* from experiments which involved such suffering."

"By the discovery of anæsthetics we have been *relieved* from that necessity, and our present *task* is to devise measures which *may* prove effectual *to prevent abuse*" (xiii.).

Here they have, as is assumed, only to defend painless operations, or that at least the greater portion may be painless; and "the severer part" of necessarily painful operations, may be done under complete anæsthesia.

But if this were true, and if the suffering, thus hypothetically abolished, were the only objection to this experimenting search for "knowledge," why should the Commissioners have the "task" of devising "measures which may prevent abuse?" What "abuse" could there be, if there were no suffering?

Does not this mean, that the existence of *Anæsthetics is not a security* for their efficient use, even in cases where they were confessedly admissible?

Here is an ominous conclusion to a fair disarming promise. These Commissioners, empowered to give advice to a "legislature, desirous of giving effect to the moral sense of the community at large," first lull that "moral sense" into a dream of anæsthetised operations; and next, re-awaken suspicion, by intimating the necessity for some "effectual" defence of the animals operated upon, against those, who, though they have anæsthetics in their possession, may possibly not use them.

All this implies that the COMMISSIONERS themselves *had* NO REAL RELIANCE *on anæsthetics, as an effectual defence* of the animal against painful and "protracted suffering."

All the legislation which they suggested, implied the same;—that "*no other* persons [but licensed physiologists]

should be permitted to perform experiments" (xx.); that magistrates ought "to be empowered, on cause shown, to authorise the police to enter and search the premises of persons suspected of performing *experiments without license*, and" that "the performance of such experiments *without license* should BE PENAL" (xxi.).

Why is this high pursuit of "knowledge important to mankind, but only to be looked for from experiments" of this sort (xiii.), to be "made penal," if pain is abolished by anæsthetics, and if pain is the only objection? The pursuit of this "important" "knowledge" should either be free and open to all, or be permitted to none.

We are assured "that, by the use of anæsthetics *in* HUMANE *and skilful* hands, the pain which would *otherwise* be inflicted may, in the *great majority* of cases, be altogether *prevented*, and in the *remaining cases* GREATLY *mitigated*, and that the infliction of *severe* and PROTRACTED *agony* is, in ANY *case*, to be avoided" (xvii., xviii.).

But what guarantee have we as to the "*humane* and skilful hands," nay, is it not considered the very essence of a humane physiologist, that he considers the welfare of humanity, and must consider the importance of his researches, rather than the price paid by his victims, as vicarious sacrifices for human welfare?

All is left to the caprice of the operator; and *his "humane" character will* lead him to *surrender and crucify his feelings* of pity for the animal, in order *to do good to mankind*:— which is the whole theory and defence of the practice.

Is there then, the public may ask, still a possibility of "severe and protracted agony" now that anæsthetics are invented, and have we *no other security* than the feelings or opinions of the operators? Do not the Commissioners say, "We have been much struck by the consideration that *severe experiments* have been engaged in, for the purpose of establishing *results* which have been considered *inadequate* by persons of very competent authority?"

Do they not add, "cases may *not improbably* arise, in future, in which the physiologist may be disposed to underrate the pain inflicted in the course of *establishing results* which may prove to be trivial or even worthless?" (xvii.).

All this shows that *the Commissioners do not trust anæsthetics*, as the defence of animals against suffering "protracted agony" even for "trivial" fancies.

They proposed to place experimenting under SUPERVISION, at least *this was the idea held forth*, as another *soporific* to the public feeling, which was to be satisfied or pacified by the appearance and profession of "submitting physiology to a system of licenses and inspections," as Mr. Simon indignantly described it (1492).

It *is* now so licensed and inspected, "after a sort"—that is, it is *screened by privacy and protected by license*, and made into a *monopoly*, which no "outsider" shall infringe upon without his offence being made "penal" (xxi.).

It is the *only line of investigation* for high, scientific, and moral ends of beneficence, which it is a *crime* to pursue, even after the discovery of anæsthetics, without a license from the Secretary of State.

But though the Commissioners did not really depend upon anæsthetics, or else would have suggested no interference for other safeguards, yet they declare that "the recommendations, which we shall humbly submit to your Majesty, will turn, *in a great measure*, upon THE USE OF ANÆSTHETICS.

The whole subject of experiments upon living animals *has been, or at least ought to have* been *relieved* of the *greater part* of *its difficulty* by the discovery of anæsthetics and particularly of the anæsthetic properties of the vapours of sulphuric ether and of chloroform, in 1846 and 1847" (Report, x., xi.).

That fatal phrase,—"at least OUGHT to have been relieved," is confession that, if anæsthetics could be trusted, when used, the physiologists cannot be relied upon to use them.

II. *The disbelief of the Commissioners in this anæsthesia apology, justified by testimony, observation, and experiment.*

No more effective solace to the public mind, and no more complete answer to the charge of cruelty in the infliction of pain could be advanced, than a proof that no pain is inflicted, or none of any substantial character. The folly or illogical nature, and the utter futility of such researches, might be maintained, but the wickedness, immorality, or cruelty of them, could not be truly affirmed.

The subject would be left to scientific men, wholly on the grounds of science, to consider the value of this peculiar method of "original research." This would be an "academic question," of considerable philosophic importance, perhaps, but not one of great or immediate public interest. We should have had no Royal Commission *to anæsthetise the public mind,* if this had not been a question of the moral feelings as well as of assumed scientific research.

We are the more concerned therefore to "verify" the value of this anæsthesia apology, and to see whether it is "a mockery, delusion, and snare," instead of a "relief from the greater part of the difficulty" belonging to the "whole subject of experiments upon living animals" (xi.).

We have already seen that those who proclaimed this "relief," and saw it clearly, so to speak, with one eye, did, with the other eye, also see, equally clearly, that it could not be relied on, and that, in fact, the same safeguards— "measures which *may* prove effectual to prevent abuse"—are now still required, as much as if no such "relief" had been afforded.

How far this is the case, and wherein the apology of anæsthesia—as a plea for the continuance of this experimenting system—utterly fails we now proceed to show, by FIVE POINTS.

1. There are a great many cases to which these anæsthetics are confessedly inapplicable.

2. There is no certainty that they will be administered in those cases in which they are by some acknowledged to be applicable.

3. There is no rule obligatory, but the convenience and caprice of the operators, as to what experiments are interfered with by anæsthetics, and to decide, authoritatively, when, and to what extent, they shall be administered.

4. There is a degree of uncertainty on the whole question of anæsthesia.

5. One agent, curari—often used for the convenience of the operator, as producing stillness or absence of motion, without diminishing sensation—is frequently used in the place of genuine anæsthetics.

Before examining these five points in order, it will be useful to notice the *tone of the questions* put by the Royal Commissioners on this subject of anæsthetics, evidently with the view of soothing the public mind, by showing that very little, if any, cruelty was involved in these so-called scientific researches.

These questions were of a general, leading, and palliative cast, such as,—Have not anæsthetics made a great difference on this subject? May not the majority of operations be performed under them, so as to be comparatively painless? And, May not the severer operative parts of those experiments — the subsequent parts of which are necessarily painful — be relieved by anæsthetics? (20-26).

The utter futility of this palliation, on the plea of anæsthetics, may be seen from considering:—

1. That there are a great many and several kinds of experiments, to which anæsthesia cannot be applied at all, according to the declarations of the "practising physiologists" themselves.

Amongst the three general sorts of experiments, as classified by the Commissioners, the *whole of the second and third class, and a moiety of the first*, are cases in which anæsthesia vitiates the result.

All operations which have to do with sensibility, as connected with the nerves of sensation, and wherein the *feeling* of the animal is a point to be observed, are inconsistent with anæsthesia.

Another sort, in the first class, admits of anæsthetics only in part of the experiment.

As, for instance, in experimenting on biliary secretions in a dog, there is first the cutting process or operation to prepare for inserting the tube in the bile duct; which part *may* be performed under anæsthetics, but afterwards the animal must come out, and remain out of that state, while the operation of some agent on the secretion is daily watched.

So, out of three classes *(a)*, those in which the knife is used or vivisection proper *(b)*, those in which drugs, therapeutic and poisonous,—are tried, and *(c)* those in which the animal is artificially infected with some disease, as cholera, fever, or tuberculosis, in order to teach the producer and observer something about the same or similar diseases in man,—out of all these, the whole of class *b* and *c*, and two sets under class *a*, are inconsistent with the use of anæsthetics.

Besides these, there is a miscellaneous class which we may call *(d)*, as drowning, hanging, and other methods of asphyxia, performed effectually in answer to the Royal Humane Society's inquiry after improved methods of resuscitation, which question was not effectively answered; also, baking, burning, scalding, boiling, starvation, and other horrible and atrocious and seemingly incredible physiological or pathological experiments, to which anæsthesia is inapplicable.

A few testimonies and illustrations—selected out of many —may serve to enforce, if needs be, *this* point—that there are many cases of experiment which, despite the plea of anæsthetics, *cannot* be conducted painlessly, while many that *can* be, are *not* so conducted.

Professor G. M. Humphry (667), examined by Lord Winmarleigh:—"Did I understand you to say that the

anæsthetic practice is *applicable to pathology* as it is to physiology?—I *think not*, because the process may have to be observed during *some days*." "668. (Mr. Hutton) *Or weeks?*—Yes, or weeks."

"670. Would you propose to make any distinction in legislative language for this difference between pathological and physiological experiments?—I would indeed. "671. A *very great exception should be made* in instances in which the process is to be *extended* and watched."

Respecting the experiments on "Recurrent Sensibility," given at page 403 in the Handbook, where the *sawing through* one or two of the vertebræ forms the *operative part* of the experiment, which is euphemistically called the "severer part," and *may be done under chloroform:* Mr. Hutton asked:—"747. But *not the other part*, I suppose?" namely, the "exposing and irritating the peripheral stumps of the anterior roots," "causing movements indicative of pain."

To this question no answer was given, except that "it would very rarely be necessary to perform or exhibit that experiment;" but to the next question (748), the answer ends with these words:—"It is *a direct experiment on the point of sensation*, and sensation, of course, *cannot be determined while the animal* is UNCONSCIOUS."

Here is a special line of experiment extremely painful, and the suffering of which is excused on the ground that it *is required* by the nature of the case. In this is involved the principle that any amount of agony is to be inflicted that is necessary for the operator's purpose. The first grand blind, the apology of anæsthesia, is thus followed by "Necessity—the Tyrant's Plea," as Shakespeare somewhere aptly phrases it.

Sir James Paget said:—"There is *a class of experiments* which are *becoming more frequent*, and which, I think, in many respects *more necessary*, namely, testing the effects of medicines and of poisons, and the production of diseases in

animals," "and *in those, of course, anæsthetics cannot* be used" (278).

Here classes *b* and *c* are distinctly and entirely excluded from the "relief" provided by the soothing discovery of anæsthetics.

Dr. Alfred Swain Taylor, so well known in connection with medical jurisprudence in cases of poisoning, said:—"1181. We require something like *an active nervous system* to show us what the effects of a poison are, whether the brain is affected, or the spinal marrow, or the sympathetic nerve, or other parts, and this we could hardly know *if we destroyed sensibility* by another poison before beginning to apply the poison in question."

The poor DOG which is said, not derisively but gravely, and in one sense truly, to participate as well as man (but no better) in the benefits of the "advance of knowledge," and which certainly shares in the sufferings out of which this "knowledge" is extracted, is specially incapacitated for chloroform, though, in experiments on his liver, he is treated to *curari*, to prevent any movements of his body squeezing out the bile, and so interfering with the investigator's calculations (2908).

Mr. Pritchard, Professor of Anatomy in the Royal Veterinary College, said:—"797. With regard to dogs I should never think of applying chloroform at all; I should think it very unsafe to do so. The dog has an intermittent pulsation; the heart's action is intermittent."

"798. They appear for some time not to be under the influence of it at all, and then suddenly they come under the influence of it, and we find it impossible to bring them round."

The Handbook for "Beginners" was noted for omitting anæsthetics from many of the painful experiments which it describes and prescribes. (See Questions and Answers in the Royal Commission, 531, 532, 584, 659, 660.)

Enough has been said to show that the *anæsthesia apology*

fails in any way to reach and "relieve" a large number and variety of painful experiments.

2. There is no guarantee of the use of genuine anæsthetics even in those cases where they are generally considered applicable. The Rev. S. Haughton, M.D., shows that the *means of rendering experiments painless are neglected*, and that THE OPERATOR CANNOT BE TRUSTED to use that, which "relieved" the task of the Commissioners, more than it relieves the sufferings of the animals operated upon (1882). "1883. I would require *the controlling power*, whatever it is, TO ORDER the anæsthesia in cases where they thought fit, and to *permit its absence* where they thought fit, but I would leave *no discretion* to the operator." "1884. I know the practice is to use the anæsthesia very imperfectly, and *when the controlling eye is gone* to drop the use of it altogether."

This is the testimony of an ardent admirer of physiological experiments—not for "demonstration," which he condemns, but for "original research," which he defends (1832).

When the Examiners resumed this question, this witness was again asked:—"1892. You stated that you had no confidence in a mere regulation that experiments should be performed under anæsthetics. Could you give us any further reasons why you think that? I need not tell you that it is *a most important* part of the matter." He replied:—"Well, *that is notorious amongst physiologists.* Unless the thing is inspected I would not trust to it that complete anæsthesia would be produced. *I would not trust the enthusiast* who is *dissecting* [a living creature] to keep it [anæsthesia] up always as long as it was needed.

Here we also learn *what sort of inspection* is required—not an occasional visit, but a constant supervision. In another answer, this witness protests *against the inspector* being an *expert physiologist*, and makes a somewhat rude and condemnatory observation as to the little confidence to

be placed in experimenting scientists, as inspectors, exercising any humane control over the processes.

So first, there are large and important classes of experiments that cannot be done under anæsthesia; and secondly, those that *can* be so done, *are* frequently, and may be always, done without such protection to the animals.

3. Thirdly, there is no binding rule, but the varying opinions and convenience of the operators and their mere will, as to what experiments are interfered with by anæsthetics, or to what extent they should be administered.

The Handbook for Beginners, by Dr. Burdon-Sanderson, Dr. Michael Foster, Dr. Emanuel Klein, and Dr. Brunton, left the matter of producing painlessness generally unprovided for.

This gave rise to the question to Dr. Sharpey:—"584. When you said that the greatest possible use of anæsthetics was always made by the physiologists, it struck me that this Handbook would hardly bear you out in that?—I cannot answer for the Handbook."

Excuses were made for the omissions and indefiniteness of that book, and the authors were advised to remove in some way the erroneous impressions liable to be produced by this neglect, or want of exact guidance "for Beginners."

In reference to the very painful experiments, "inflammation of the cornea, produced artificially by the nitrate of silver" (659), described in the said book, Professor G. M. Humphry observed:—"I do not think this account at all indicates that it is *not* done under anæsthesia. The fact of the animal being under anæsthesia *is usually omitted* in this [Hand] book, it is *taken for granted.*"

This was the best apology for the carelessness on this point of sensibility so characteristic of that guide to "more precise" knowledge.

Dr. Rutherford, being asked (2841), "*What is the rule* by which you guide yourself in determining whether they

[the animals] shall be rendered insensible to pain or not?" said: "*When* the mode of *rendering them insensible to pain would interfere with the due result*, being obtained from experiment, we do not so render them."

2842. "Is that any large proportion of the experiments? —I should say a *considerable* proportion."

The rule of *convenience*, as to whether anæsthesia is or is not resorted to, seems generally adopted, as is intimated in the following answer by the Rev. S. Haughton, M.D.:—

"*If* it [anæsthesia] would *facilitate* his [the experimenter's] operation it would, *of course, always* be used by him, but *not* always if it would *not facilitate* it."

Now this "facilitating the operation," as the convenience of the operator, not the relief of the victim, is the defence given for the using of curari, which is no anæsthetic, but a *quietener*, as Dr. Sharpey observed of it, that "it facilitates the operation *at any rate*" (463).

So that, in point of fact, it all comes to this — the operator's convenience, and his personal opinion as to the best way to gain his "results." There is no rule, either of authority or of scientific distinction; acordingly, Dr. Pavy thus answered question 2178: "Would it be possible to *draw a distinct line* between the cases in which chloroform cannot be used and those in which it can be used?—*That* MUST BE left to the physiologists; there CAN BE NO DISTINCT LINE DRAWN, I think."

Thus we are again cut off from the retreat into the covert of anæsthesia, as a defence of these operations, since there is not only a great number of cases to which all confess anæsthesia is inapplicable, but also there is no certain rule as to those cases in which it is required or imperative.

Dr. Rutherford said (2882) "that, if legislation were thought advisable"—which few physiologists spontaneously agreed with, and which most repudiated—"then I believe that the Government ought to license certain men to carry on vivisection, BUT OUGHT *to leave the details of the manner*

in which the vivisection is to be carried on, to the persons licensed."

This is practically the case at present, although the *form* of inspection, by the appointment of an inspector who can be only in one place at a time, is established as a concession to the public, and whose appointment is a proof that anæsthetics are no security for painless operations, otherwise an inspector would not be required. But since there is no rule as to what operations should or should not be under anæsthetics, the inspector may well, and with safety, inform the Secretary of State, as he is lately said to have done, that he is perfectly satisfied; for there is no rule to be broken, and so no complaint to be made.

According to Dr. Pavy, no rule could be consistently imposed on a science which is carried on by the will of the working physiologists, and is as liable to gusts and changes as the atmosphere under whose sudden veerings the *Eurydice* went down. He said (2179): "*No line can be drawn*, simply because you cannot foresee the kind of experiments which are likely to be required. As *knowledge advances* we experiment in a new line."

"I think," says Dr. Rutherford "it would be *very difficult to define* exactly what experiments should be done under anæsthesia, and what should not" (2882). What one thing, on any point, *do* they define? Is not every phrase equivocal?

Thus far, we have seen, that there are a great many cases to which the apology of anæsthetics does not apply; that there is no guarantee for their being applied where they could be, and there is no line of distinction as to what cases anæsthesia may or may not be properly withheld from.

4. To these difficulties "in the one scale," weighed against the alleged relief of anæsthetics "in the other," we add, in the fourth place, *that there is a degree of uncertainty* on the whole question of anæsthesia.

It is not only uncertain to what cases anæsthetics may be

and ought to be applied; but there is *an uncertainty as to whether, when they are applied, they work efficiently* to produce complete painlessness, and how long this continues if produced.

Dr. Rolleston said:—"I have tried myself a number of experiments upon the action of anæsthetics, and" "*it is not so easy* a thing *to know when* you have an animal *thoroughly anæsthetised;* and what is more, some animals recover with much greater rapidity than others, of the same species, from the same doses of anæsthetics."

Such considerations must strike with some dismay those who may have been led to dream that "the whole subject of experiments on living animals has been, or at least ought to have been, *relieved of the greater part of its difficulty* by the discovery of anæsthetics" (xi.)

These anæsthetics themselves are a difficulty; added to the other difficulties of their inapplicability to many cases of suffering, the uncertainty whether they *are* applied where they *might* be, to what cases they are really applicable; and *now* the difficulty is—whether, *when* applied, they are *reliable.*

In dealing with the scarcely doubtful case of curari, Dr. Rolleston turns off to anæsthetics again, and says:—"1350. But I must say that the WHOLE QUESTION *of anæsthetising* animals has *an element of* UNCERTAINTY about it. Some animals are, some are not, within the limits of the same species, amenable to anæsthetics just as we are ourselves."

The scientific certainty which physiology is to introduce into medicine, is scarcely a quality of this so-called physiology itself, which, like some animal deprived of part of its brain, reels and staggers with a certain want of "co-ordinating power," as the brainal physiologists say of their unhappy mutilated subjects.

Dr. de Noé Walker gave the finishing stroke to the anæsthesia apology, in his frank and decisive way. The question to which he replied was, perhaps, a leading one,

viz.: — Whether anæsthetics were generally used in this country? and the meaning suggested was, that such use of anæsthetics, if admitted, was a great palliation of the system of experimenting.

Therefore, the witness answered the meaning and motive of the question rather than its bare words.

"1810 (Mr. Forster). Have you any opinion as to whether the experiments which are made in this country are generally made with anæsthetics, or not?—The question of anæsthetics is not so very simple. When an experimenter says, for example, as is said in a very recent publication, that 'before and throughout these experiments anæsthetics were used,' it is perfectly true; but if by that you choose to understand that, while the animal lived and was experimented on, he was throughout insensible, it is the *greatest delusion* that ever was." So that whether anæsthetics are nominally used or not, there is no certainty that the animal is really anæsthetised.

We have thus advanced four propositions, which destroy all intelligent confidence, in the plea that anæsthetics for the most part abolish suffering, or reduce it to an infinitesimal quantity.

5. Let us now notice, fifthly, the case of *curari*—sometimes, perhaps, used as an anæsthetic, although it is not known to be one, but generally acknowledged to be an agent that paralyses the nerves of motion, without deadening the nerves of sensation. It is more than "suspected" that much cruelty has been practised with this agent, under whose influence the animal is perfectly still, and may, *to a "Lay" spectator, seem in a painless condition*, while suffering agony, of which its condition of paralysis prevents any expression.

The danger to the suffering victim, from this convenient agent in the hands of the operator, is not imaginary, and may well excite dismay in the hearts of those who have

relied on the production of insensibility as a "relief" to their feelings with regard to experiments on living animals.

Sir Thomas Watson, while taking a more favourable view of English physiology, evidently, by the exception, admits that *Foreign* physiology resorts to the "FRAUDULENT TRICK" of passing off *curari* as an anæsthetic; and so, while pretending to avoid giving pain to the creature operated upon, only conceals from the bystander the agony that is inflicted.

This "trick" is expounded in the following questions:—

"75. Can you tell me anything about the particular action of *an anæsthetic* (!), which is sometimes called curari and sometimes woorari?—I know it has been *said* that the woorari poison, which renders the animal quite unable to move its limbs, *has been used to veil*, and so to *conceal from the observers*, the pain which the animal, nevertheless, suffered; but that any such *fraudulent trick* has ever been practised by the physiologists of *this* country *I decline* to believe.

"76. I do not imagine that *that* is what is usually stated, but that it has been used to keep the animal perfectly motionless. Are you acquainted with this 'Handbook for the Physiological Laboratory,' edited by Dr. Burdon-Sanderson?—No; I have never seen it.

"77. In this Handbook it [curari] is constantly named as being used, and apparently for the purpose, for instance, in experiments on the arterial system, of *preventing* the failure of the experiment by *the motion of the animal*; but I find the strongest possible evidence in Claude Bernard's books, that it has no effect whatever on the nervous system,—that it simply paralyses the motor system?—If that is all it does, then it does *not enhance* (!) the animal's sufferings.

"78. It simply renders it motionless, and leaves the suffering alone?—And would *facilitate*, probably, the experiment itself."

What Sir Thomas Watson generously "declines to believe"

about the "physiologists of *this* country;" is not only indirectly admitted respecting those of *other* countries, but it is proved that, whether those of this country profess to use curari *as* an anæsthetic or not, it *is* used, according to the descriptions of the Handbook, *in the place* of an anæsthetic, or in such cases as the public might suppose the discovery of anæsthesia had covered and defended. That it "would *facilitate*, probably, the experiment itself," makes it the *more dangerous* to the animal, since, as we have seen already, the operator is disposed to use what would "facilitate his operation" (1893) rather than what would relieve his victim.

There are some who even think that curari *intensifies* the feeling, while it paralyses motion; and it is certain that this agent is extensively used as, at least, a convenient *quietner;* and sometimes—to cover this convenience, perhaps — it is even professedly regarded as probably an anæsthetic.

For the "certainty" of this physiological science, leaves uncertain the character of an agent which it constantly uses.

Sir Thomas Watson, however, was too honourable to fall back on any such resort as the *possible* anæsthetic virtue of curari.

Accordingly, when asked, he answered thus frankly:—

"81. *(Chairman.)* In your judgment it is *not* an anæsthetic, is it?—I imagine it is not an anæsthetic at all.

"82. And to treat it as an anæsthetic is a FRAUDULENT TRICK in your view?—It would be.

"83. Then, in short, *you have described that as a fraudulent trick?*—If it *were* used *merely* to deceive bystanders into the belief that there was no pain suffered, I should call it a fraudulent trick; but it *might* be used for a much better purpose—namely, to facilitate the aim of the operation."

True it "might," and it might be used for *both* purposes as a double "convenience." But whether this agent is purposely used to deceive the on-looker or not, it is *not* a

relief to the animal, though it looks like an anæsthetiser by the quietness it produces.

All these uncertainties as to the methods used for "facilitating the aim of the operation," leaving so many occasions in which painlessness is not produced, makes the whole apology of anæsthesia practically, if not morally, "a fraudulent trick," as "leading the public into the belief that *there was no pain suffered.*"

For *this* is what the apology *means;* and it is not true. In a large number of cases, as already seen, anæsthetics are declared inapplicable; in other cases their use is merely at the convenience of the operators; and here is an agent, curari, which meets this convenience, but does not give that relief which is assumed in the plea to be actually secured.

The limits of this Essay do not permit further details in proof that curari *is* used as an anæsthetic, "and is called an anæsthetic" (462); and that this "fraudulent trick" is practically played; but the following instance may indicate that the "*public feeling*" against suffering *is mocked and soothed by this delusion,* which of itself illustrates *the practical deception* of the *whole* APOLOGY *from anæsthesia.*

Addressing Dr. Sharpey, the Chairman inquired:—"437. Then if experiments *prima facie* painful are performed upon animals with *no other security* against pain than that of this particular poison [curari), we have no *scientific* security at all that the end is really attained?—No, we have no security that the sensation of pain is abolished."

After a few questions as to whether it is a "mere chance," or a "probability," and the declaration that Claude Bernard's great authority is decidedly against the anæsthetic character of curari, the Chairman asks:—"441. That being so, is it *legitimate to hold out to the public the notion* that *experiments of this kind, performed under the woorari poison, are performed under an anæsthetic?*—I should explain to your lordship that *I never held* the opinion that curari took the place of an anæsthetic.

"442. I *never* supposed that you did. I had an object in putting the question, which I can explain to you if you like?—I wished simply that what I said might not appear as said in support of the notion, that curari was a *sufficient* anæsthetic. I was merely dealing with a *nice question* in physiology.

"443. *If the public have any rights at all* in this matter, to have their feelings respected, those *rights are not regarded* when the experiments performed under the woorari poison are *held out* as experiments *performed under an anæsthetic?* —No, I think not. I think that *that* is not a *sufficient* answer to the public."

Here it is clearly implied, first, that the "public feeling" or conscience of the country had led to this inquiry; secondly, that the public had some "rights" to demand that its feelings should be respected; thirdly, that such "rights" were not to be set aside by any claim to independence of action, so often put forth by the physiologists; fourthly, that these *rights were mocked,* instead of "respected," by the insult of a pretended anæsthetic; and fifthly, that some more satisfactory and "sufficient answer to the public" was demanded.

Now, *the same is true* respecting the ANÆSTHESIA APOLOGY, TAKEN AS A WHOLE; it is *as delusive* as the curari fraud, which is a part of it; and if this apology of anæsthetics is not *intended* as fraudulently it is still equally *delusive* and unsatisfactory.

A variety of forms of apparent torture is systematically inflicted upon animals, and *one of the excuses* is, that they are put into a state of insensibility, and so are already rescued from the sufferings that would otherwise be endured; yet, when we inquire carefully into the truth of this apology, we find:—

Firstly, a series of painful experiments, in which insensibility is impossible, as interfering with the purpose of the experimenters.

Secondly, that where insensibility *might* be produced, without so interfering, there is no certainty that it *will* be, but grounds for suspecting that it is *not.*

Thirdly, though we have a clear line as to many classes of experiments where anæsthetics *cannot be* applied consistently with the operator's purpose, there is no line of distinction respecting the remaining classes of experiments, as to which of them admit of, and *demand* anæsthetics. So insensibility is positively excluded from *many* cases, and is left wholly uncertain as to *all other* cases.

Fourthly, the whole question of genuine anæsthesia is wrapt in scientific uncertainty, and like the special case of curari "is a nice question in physiology" (442), which abounds more in questions than in answers.

Fifthly, this agent, curari, is used *as,* or in the place of, an anæsthetic, though it is not one; but is diligently sought to be defended as possibly, or to some extent, an anæsthetic, in order to escape the charge of "a fraudulent trick."

This case of curari is but an illustration of the deceptive character of the entire plea about the insensibility under which, as is alleged, the majority of experiments may be performed.

That scientific authority was therefore not without good reason for his opinion, as quoted by Mr. Hutton:—"534. I have read an opinion, expressed by a respectable physiologist, that *so far from* anæsthetics having been *really a blessing* to animals, for the purpose of these experiments, they have *removed the odium* of the experiments, and only in *a very slight degree saved* the animals from pain, and certainly not the lowest order of animals."

But now, when the whole scheme is expounded, the fallacy of the whole pretence brought clearly out, "THE ODIUM" will be re-doubled, and the public feeling, first outraged, will become more intense through an intelligent appreciation of what, in fact, though not in purpose, is "*a fraudulent trick.*"

CHAPTER XI.

THE PLEA OF NECESSITY, AND CLAIM OF ABSOLUTE LIBERTY; OR, THE SURVIVAL AND EXTENSION OF PAIN AFTER ITS PROFESSED ABOLITION BY ANÆSTHETICS: AND THE JUSTIFICATION OF INFLICTING ANY INTENSITY OF AGONY; ANY CONCEIVABLE FORM, OR ANY POSSIBLE PROLONGATION OF PAIN AS NECESSARY FOR THE PURPOSES OF THE EXPERIMENT, AND THEREFORE "LAWFUL AND RIGHT."

IT is claimed for the operator that he is the sole competent judge as to the kind and method of experiments; and the only province of the Legislature is to "protect the professed physiologist" against any penalty for what is done within the sphere of his absolute discretion.

Sometimes it is argued that very little, if any suffering, is *necessary*, because it can be avoided by anæsthetics; but now, the very NECESSITY for suffering, which is *denied* in the other plea, is *advanced* in this as a justification of that suffering.

The question for the Royal Commission was, how to reconcile popular feeling with alleged scientific inquiries.

The great reconciler upon which, as the Commissioners acknowledged their "recommendations" "in a great measure" turned, was "the use of anæsthetics" (x. xi.).

How delusive that is, we have shown at length.

But behind this plea, of the painlessness of the majority of operations, rises up the startling *plea of the necessity* for inflicting any amount of suffering, in any number of cases, according to the opinions of the operator, as to the importance of his aim in the experiment.

Here, instead of the soothing assurances—that operations are for the most part painless, or that "the severer parts" are rendered so by chloroform, that the "subsequent" pain,

where any is essential, is "not great," or "not protracted," or "lingering," by which public feeling was "narcotised"—we are boldly confronted with the declaration, in every form of emphasis, that all pain is justifiable, which, in the opinion of the "practical physiologist," is required for the success of his experiments.

This again is frequently softened by some "nice physiological question" as to what is really painful, and even whether there is such a thing as "pain" at all. But if there is no pain, as several "physiologists" intimated to the Commissioners, then the pretence of assuaging human suffering by physiological discoveries, becomes itself ridiculous.

This intimation that there perhaps is no pain, and that what we shrink from in horror, or look on with pity, is imaginary suffering, was advanced to palliate the most repulsive experiments. But, at this stage of the argument, pain is admitted and justified to any extent. Public opinion is resented as an intrusion on the sacred territory of the priesthood of science—"the highest teachers of our race."

(5487.) No "weak sentiment" (5476) is to "govern" the acts of "that highly elevated and excellent portion of the community" (5483) which "puts A LAMP, *so to speak*, into the hand of the physician" (394), and to regulate whose course, by any public law, is to forget that "those laws would be made for the ignorant, and not for the best [or more enlightened] people in the country" (5482). This kind of assumption, so freely indulged in, will, when understood, be as distasteful to the public, as it is to cats to be fed with colocynth; "although," as Mr. Huxley acknowledged, "it is not a nice thing to force colocynth into a cat's throat" (4891).

What the pre-eminently, if not exclusively, "scientific" class requires, is, absolute license on the following principles:—

1. That no extent of suffering should prevent the performance of any experiments with "a scientific purpose," to which experiments that suffering is necessary.

Dr. Burdon-Sanderson lays down the rule that "the

question of right or wrong depends upon the relation between the *purpose* of the experiments [or experimenter] and the pain inflicted;" that "if the purpose is a good purpose, and" if "the experiment is made in the most skilful way," and "no pain *not necessary for the purpose* be inflicted," "the whole thing is a right action" (2750).

In other words, any one who protests that he means well, and who sets before himself what he calls a useful object, is right in inflicting whatever pain may be necessary for his experiment. Of course *all the experiments*, however cruel or however absurd in relation to their object, *are done with "a good purpose,"*—the profession to advance science, or to help humanity, or both in one.

When the question was put to Dr. Sanderson, whether the conduct of Sir Robert Christison—who having commenced an experiment "for a sufficient purpose," desisted in the middle, on account of the agony caused to the animal—was "an amiable weakness," or "a conscientious act?" he preferred not answering, as "not remembering the case" (2753), though he had just described it "as an experiment upon some irritant poison" (2752).

Pressed further, as to whether it is right to retire from "any experiment where" the operator finds he is inflicting extreme pain, he answered:—"I think it would *be foolish to do so*."

THIS MAKES PITY ITSELF FOLLY, and requires that *any pain*, necessary for the scientific or imaginary "purpose," must, as a matter of duty, be inflicted. The same rule of a "right action" is accepted in a positive form as follows:—"2763. As regards research [in opposition to "demonstration"] you would consider that *any pain*, supposing it was sufficiently justified [by "the purpose"] might be inflicted? —*Yes*, with the proviso, that we ought constantly to have in view the object of making it [the pain] as small as possible."

This "proviso" is indefinite and irrelevant; and even this "proviso" is omitted by one of Dr. Sanderson's co-

editors of the Handbook, Dr. Klein, who, with a frankness that did him credit, as distinguished from the verbiage and ambiguity of some other professors of "pure science," went straight to the point, and spoke as if he were "*practical physiology*" *embodied;* declaring that his only concern was to get his experiments done as quickly as possible, and to get out of them all the scientific results possible (3540). He affected no sympathy with the animals. Some have severely blamed Dr. Klein for his out-spoken evidence,—that he did not regard at all the sufferings of the victims, but only the "purpose" of the experiments. What is this but *the very essence of the experimenting position*—that the pain of the animal is necessary for the objects of the practising physiologist? Everything is subordinate to these objects.

"Except for teaching purposes, for demonstration," he says, "I never use anæsthetics, where it is *not necessary for convenience*" (3538).

"When you say that you only use them for convenience' sake, do you mean that you have no regard at all for the sufferings of the animals?—*No regard at all*" (3539).

This is modified in Dr. Klein's correction of his evidence, which the Commissioners declined to accept, but printed it at the end, in Appendix II. There he adds to "No regard at all," "*for such little suffering* as is in my operations." In his evidence he said:—"An experimenter, or man who conducts special research," "has no time, so to speak, for thinking what the animals will feel or suffer" (3540).

This is what practising physiology assumes fundamentally —that whatever suffering is *necessary* for its object *must* be inflicted.

Dr. Burdon-Sanderson says of any one who, from pity, turns back from an experiment, "It is foolish to do so" (2754).

Dr. Ferrier, when asked whether there might not be suffering so great that certain so-called "physiological results" had better "be dispensed with, than attain them by these exceedingly painful experiments—I gather you do not

think that?"—answered, "I think we are in need of every additional fact to advance the science of physiology" (3393).

These "facts," as the confused jumble of "results" is called, are considered more important than any suffering.

When asked:—"Do not you consider that the sufferings of animals are sufficiently important, as compared with the sufferings of human beings, to make it often desirable rather to sacrifice a physiological truth than to inflict all these sufferings?—No; I do not see that the sufferings of animals are so very great, as compared with human sufferings" (3394).

This is a sufficient indication of *the tone of sentiment* on which practising physiology is founded, that the purpose of the experimenter justifies any amount of pain that is necessary—that "the end sanctifies the means;" though "the end" is generally missed, or else we should not, after all these "means," be still so sorely "in need of every additional fact to advance the science of physiology" (3393).

Every one is presumed to have a "good purpose," which makes the experiment a "right act," including such "acts" as the alcoholising of a dog at Norwich.

On that case, Dr. A. S. Taylor observed:—"I read in the medical journals what did pass about some injections that were made into a dog. To me they appeared to be *of a most cruel kind*, and to answer no purpose justifying the nature of the experiment. Of course there will be a difference of opinion always in such cases, and I think *you will find generally that physiologists will maintain a right to experiment to* ANY DEGREE."

2. The second point for consideration here is that, while the importance of the purpose is in general held to justify all pain in carrying it out, there is no rule as to what *is an* important purpose, and no settled principle for testing it. So the ENTIRE EXCUSE, on which the right of inflicting suffering is built, collapses.

Even physiologists themselves may not judge as to the

sufficiency of the objects for which "brother physiologists" undertake a series of painful experiments; each being supreme autocrat in his own laboratory, and his own "conscience" being the sole rule as to what is or is not a "sufficient purpose," or a reasonable experiment (1485).

One of them being asked as to the sense and propriety of some questionable investigations, perpetrated by another (2405), modestly declined to venture an opinion. His answer in the case was very much like that of a good woman, who, having been praising very highly her favourite preacher, was asked, "Do you understand him?" and answered, "Do yè think I would *preshume?*" Such is the distant and reverential awe with which our "home-bred physiologists" regard their Continental masters.

When the physiologists place the *importance* of "the purpose of their experiments" against the failure of good results, and the amount of suffering inflicted for nothing, it is not to be presumed that any "outsider" (5489), or insider either (3323), can judge as to the failure of the attempt, or "the importance of the purpose."

Thus, Dr. Sharpey laid it down as a rule, that "scientific men" perform "vivisectional experiments" "with great reserve," and "avoid experiments which are severe, *unless* when they are absolutely *necessary* for a SCIENTIFIC object" (445). Whereupon Mr. Hutton, who might have been supposed to understand the subject, even in its esoteric recesses, imagined that now, at least, a "principle" was acknowledged which could be expressed in plain English, by which the practice of experimenting could be regulated. But he was somewhat baffled, as in the following question and answer:—

"446. *This principle being*, that no experiments should be resorted to, except where its *necessity* for SOME USEFUL PURPOSE can be *proved?*—The question is, *What* IS a useful purpose?"

This was a sharp turn, and proved that "a useful purpose"

was like many of the indefinite phrases which are passed off by the physiologists as having a meaning, till the inquiry, in some necessity of the argument, is made, What *is* "a discovery?" what *is* "a living animal?" what *is* "demonstration," as distinguished from "investigation?" what *is* a "beneficial" result, as distinguished from a "scientific one?" and then all their "more precise" language and temporary distinctions and definitions vanish. So that the enquirer, who seemed to grasp a "principle," or some idea, is cheated with words only.

Thus, every form of suffering, is condoned by the "useful purpose" of the experiment; and when you adopt that, and ask, Then *only* "useful purposes" are legitimate? you learn that "a useful purpose" is an accommodating phrase, to be interpreted by the exigencies of the experimenter himself. We neither know what is a "useful purpose" nor what is a valuable "result."

Mr. Huxley, in reference to Dr. Hoggan's doubt respecting the importance of Paul Bert's curarised dog experiments, observed:—"It may be *you* may not think them [the results] important, but I think you will not deny that *it may be a fair thing* for M. Bert, *who made the experiment*, to consider them of importance" (4164).

This is the very chivalry of argument, and indicates that each experimenter is the sole judge of the goodness of his "purpose," and the splendour of his "results."

3. The third point for consideration is the principle laid down by physiologists, that experiments which seem "barbarous and horrible" to "well-educated persons" and the general public, are "perfectly right and necessary," when we regard "*the drift* of the experiment;" and that experiments which, to the public eye, "may *seem*," through the use of curari, to be "very innocent," may be really "very horrible." As to "*the drift*" of the experiment, nobody understands *that* but the experimenter.

A great deal has been said about the propriety of appointing inspectors of laboratories, and whether such should be physiological experts as representing science, or persons of general education and intelligence to represent the public in the interests of humanity.

Some have maintained that a professed physiologist would be unacceptable to persons engaged in "original research," because these inspectors might gather and steal the ideas of the experimenters, and forestall them in the market; so as to rob the investigators of those laurels which are supposed to be as much a part of their "purpose" in experimenting, as is the advance of science or the good of humanity.

It was even affirmed and repeated, to the Royal Commissioners, that original experimenters are extremely jealous, and that this would make a physiological inspector obnoxious.

Mr. Huxley, in a speech-question (3108), suggested that, if the proposed inspector "is a competent judge"—*i.e.*, a physiologist—some would regard him as "in sympathy with the scientific person who was in the laboratory, and his inspection therefore would be [considered] of no good. If he were an incompetent person"—*i.e.*, not an experimenting physiologist—"on the other hand, *however well educated a person he might be*," he would "be *unable to* comprehend the DRIFT of your experiments, and might *very often* report that you were doing *barbarous* and HORRIBLE THINGS, when you were doing that which was [on the experimenting hypothesis] *perfectly right and necessary*."

This apologetic question is a confession that experimenters "*very often*" perpetrate what, in the opinion of a "well-educated person," is "*barbarous and horrible*," but what, to a physiologist, is "perfectly right and *necessary*"—namely, for "the drift of the experiment," which is assumed to be so recondite that "well educated" people, not versed in the mysteries, could not understand. "In fact," as Mr. Huxley suggests in another teaching-question, "Physiologists would be *incessantly worried* by changes that

had no [physiological] foundation (3111)?" meaning that, what is popularly cruel is scientifically justified, by the purpose or "drift of the experiment;" and that this is "*incessantly*" carried on.

Mr. Hoggan, in reply to the same questioner, naively said:—"I am quite of your opinion that there would be an objection on the part of vivisectors to be overlooked" (4076).

When he was pressed whether it was not reasonable to entertain this "objection to be overlooked," he answered:—"I do not think it *is* reasonable, looking at the whole thing in a *just* light" (4077).

With the *usual want of analogy*, displayed in the defences and discoveries of experimenting, the questioner plied the witness with this comparison: "4079. It never occurred to you to suggest, did it, that some similar regulation [for overlooking] should be carried out in the operating theatres of the great hospitals and schools of medicine?—No; because the people operated upon *are operated upon by consent.*" This was a noble as well as logical answer; and Mr. Hoggan might have added, "the people in question are operated upon *for their own good*, for maladies occurring to themselves in the natural course of Providence, and are not the victims of artificially-produced diseases for scientific amusement."

Perhaps the most fatal, unintended, acknowledgement of the agony inflicted in these "laboratories," and of which the public and "well educated persons" are no judges, is that involved in another question of Mr. Huxley's:—"4349. That is to say, in point of fact, *very innocent things* might be done which would give them [the public] the impression that *very horrible things* were being done; and, on the other hand, *very horrible things* might be done which would give them the impression that *very innocent things* were being done?—Undoubtedly."

This means, first, that what is "barbarous and horrible" *per se*, as in question 3108, is relatively "right and neces-

sary" for scientific purposes; that what to popular apprehension, and in itself, is "very horrible," is, on scientific grounds, "very innocent;" but that the unscientific public, looking only at the suffering of the animal, and *not at the scientific speculation* of the operator, may mistake innocence for barbarity.

Secondly, this means that the public might be deceived in another way—namely, by the cunning use of curari, under which "very horrible things *might* be done, which would give them the impression that very innocent things were being done."

What does all this prove? It shows that the public would be *right* in its indignation at the undisguised cruelty, and wrong only in its condoning disguised cruelty; and therefore it would be perfectly wise to forbid the first and *not to trust* the second.

Such argumentation, fairly analysed, can produce nothing but *instinctive suspicion* respecting the whole system.

It teaches people to suspect that even when the animals do not *seem* to be suffering, "very horrible things" are "being done;" while "even" a "scientific person" may take leave to doubt whether, when "very horrible things" are obviously "done," they are rendered "very innocent" by "the drift of the experiment," or "high purpose" of the explorer.

We cannot be surprised at Mr. Lister's opinion that "of all undesirable things the *most undesirable* is, *that the public should witness these things*;" and that "no advantage"—*i.e.*, to the physiologists, but only to the animals, and to humanity—"could possibly be derived, that I can see, from the public witnessing these things" (4348).

So *draw the curtain:* and carry on these "very innocent" experiments under no eye but of Him Who is patient, but not unobservant; Who can be excluded by no license, and "from Whom no secrets are hid."

It may be fairly doubted whether, in the eye of an

enlightened unsophisticated conscience, or in the eye of the Omniscient Witness and Judge, the principle will hold good —that what in itself is "barbarous and horrible," is relatively to "the drift of the experiment," "right and necessary;" and from being "very horrible" is thus made "very innocent."

4. That experimenters object to inspection, and ask for protection; that they may be shielded by the Legislature against any penalties for cruelty, and left at absolute liberty to carry out their ideas without interference or control.

The experimenting method is, according to the physiologists, to be open to everybody, because no one can say who is competent or who is not.

On the suggestion, by the Chairman of the Commission, that Government, or some authority, might be *permitted* to prevent incompetent persons from experimenting, Dr. Ferrier said: "It would be so extremely difficult to ascertain or to determine who *were* qualified, or to *frame a standard* of *competency* to make original research, that I believe legislation in that direction would be injurious" (3253).

Again he averred: "I have no objection to a restriction being put on unqualified persons, *provided* you *establish a sound criterion* of the ability to perform experiments" (3262).

It may be observed here, that the principle involved throughout is, that each man judges of his own fitness, and that to restrain any persons would be to obstruct science. The *present licensing, by forbidding unlicensed operators*, is in the teeth of the "evidence," which, from the physiologists generally, went in favour of *unrestricted liberty of experimentation*, for the advance of knowledge and consequent improvement of medical practice, which is invariably assumed to be solely dependent on discoveries by experiment.

When Dr. Ferrier is asked (3272): "Should you, as a

practical physiologist, consider it any *stigma* upon yourself to have some limitation put upon the practice of these experiments"—"some limitation either of inspection or in some other manner?" he replies: "I should certainly *object to inspection of* ANY *kind* in *my* experiments." He added that, "a *license simply, without any inspection,* or *the necessity of keeping a record,*" might possibly be permissible.

This objection to "inspection of any kind," or the "necessity of *keeping a record,*" which might be overhauled, was natural on his part; and the "license, simply," was in order *to protect the physiological experimenter* from the law against cruelty to animals.

His reason for this "relief" to the experimenter was twofold: first, the public was angry at the cruelty supposed to be inflicted; and, secondly, it was not cruelty at all, but only agony inflicted for a scientific purpose. Hence, he pleaded for a law that should *distinguish scientific agony from vulgar cruelty,* and be a SHELTER under which "a man of science" might do what the unlearned would regard as "very horrible things"—to use Mr. Huxley's phrase (4349)—and, by virtue of "the drift of his experiment," be exempt from punishment. So, in reply to the question (3273)—"You are, on the whole, of opinion, as I understand, that the *practice* of research, by experiments on living animals, should *be open to the public without restriction?*"—he does not say "yes," for *that* was unnecessary, but he boldly advances the plea, that such "open," unrestricted freedom should be guaranteed by a law repealing the application of the penalties against cruelty in all cases of professed researches by "experiments."

His words are remarkable, and indicate *the whole tendency and claim of the physiological profession* to unrestricted operations.

He said, "Well, I think, *owing to the great agitation* that has taken place in the public mind so recently, *it is very desirable* to distinctly *dissociate experiments* for the pur-

poses of original research *from cruelty to animals*. I think that the two are totally different, and that experiments for original research do not come, in any shape or form, under the provisions of the enactments against cruelty to animals; but, as *the popular mind* has been so excited in regard to this subject, I think that some legislation, in which a *clear distinction should be drawn* between the two, would be advisable" (3273).

This means—as is claimed by other professors of the "art"—first, that the *public has nothing to do with the matter*, and has no "rights" respecting it; secondly, that the *sole province of the Legislature* therein *is to protect the experimenter* against the law forbidding cruelty to animals, and against public opinion, which is treated here and elsewhere as an unauthorised intrusion.

Some had suggested confining experiments to licensed laboratories; but it was claimed that they should be allowed ANYWHERE, BY ANY BODY, TO ANY LENGTH.

Mr. Erichsen asked Dr. Ferrier:—"3326. Would you see any objection to *places* being licensed in which experiments might be performed?—*I should object.* I should allow *everybody liberty* to perform experiments *in his own private* laboratory."

Here we have it distinctly:—"I should allow *everybody* liberty to perform experiments in his own *private* laboratory."

This was what the physiologists claimed; interference with which, by an obtrusive public, was sternly protested against.

Mr. Lister, having said that legislation would "hamper in an undesirable manner such investigations," and that "*if* it *would throw no impediment* in *the way*" he should have no objection, was next asked:—"4301. If there were no impediments thrown in the way, that is to say, of" "*the most scientific* and educated persons?—It is sometimes *difficult to define* who are *the most scientific* and *competent* persons."

He is not satisfied with "no impediment in the way of the most scientific and educated persons;" he wants "no impediment in the way of" *any person.*

We are often put to some confusion by the *argumentum ad modestiam*—do you venture to controvert the opinions of "the most scientific and competent persons"—experts in physiology?

But now, from many of these statements in evidence, there is as much difficulty in settling who are "most scientific," as in deciding—what is "a discovery."

As nobody can judge of competency, so no one can judge of incompetency. Mr. Turner was fairly drawn in by the first of the following questions, and backed out in answer to the second:—"3041. Is the number of persons who, in *your* judgment, could usefully perform experiments of this kind very great?—Not in this country."

"3042. Would the restraint of such *persons* [meaning the others] that is to say, incompetent persons, receive your approval?—Then would arise *the question,—who* is to *judge* of the competency or incompetency of the person."

It is true this gentleman himself had, by a slip, expressed his *own* judgment that there were few "in this country" who are competent; but he recalls the idea, and falls back on the "general principle" of physiologists—that nobody can say what competency is. There is the same uncertainty in this as in every other subject and phrase of this "exact science."

But though no one can decide who is competent or incompetent to pursue this "research," Government is asked to protect these experimenters, of whose capacity there is no criterion.

5. None but practising physiologists can judge of the value of physiology as an experimenting system.

In reply to the suggestion, by Mr. J. Colam, Secretary to the Royal Society for the Prevention of Cruelty to Animals,

that experiments should be "reported," Mr. Forster asked:—"1668. You mean that it would give *that publicity which would enable you to form an opinion* of the *value* of the experiments?—Yes."

Whereupon another questioner claimed for these pursuits a position above common comprehension:—"1669 (Mr. Huxley). But I *do not see* how *any but experts* can form any opinion as to their *value*. I want to know whether *that* is one object of the Bill, to constitute the Society for the Prevention of Cruelty to Animals a sort of court of revision in these matters?—Do you *not think*, I may ask, *the Society would be able to judge whether*, for instance, the experiments referred to by *you had prevented sheep disease* or cattle diseases or not, *if those diseases disappeared by* the application of the remedies discovered by the experimenters?

"1670. *No, I think not.* I think that if the Society contented themselves with reading Dr. Klein's memoir, recently published, unless I am much mistaken, there is not one in 500 who would understand anything about it?—Not the technicalities; but *the general results* we might understand. I hope so, *else* I think the experiments would be of *no great advantage* to humanity."

Now, it is possible few could understand Dr. Klein's "memoir," but that would not be wholly the fault of the reader, perhaps; and still fewer can understand Professor Huxley's assertion that—"if those diseases disappeared by the application of the remedies discovered by the experimenters," he "thinks" the Society for prevention of Cruelty to Animals would "not" be able to judge of the fact. Certainly the Society is *in no* great danger of being put to the test; for there never was a more manifest failure than in the attempt to bolster up experimentation, by the evidence of what it did *not* do, for animal diseases, as we have already seen.

But if people cannot even judge of the *results*—the great benefits conveyed by experimenting—why are these benefits

paraded as a reason for permitting and defending these mysterious benefactors of mankind?

Mr. Colam's answers are very admirable: either we can understand what good is done—if it *is* done—or else we should not be asked to protect cruelty to animals, on the ground of its innocence, as a way of conferring some good, which we cannot judge of.

That there were people, even connected with the Society in question, who could judge both of the *practical and scientific* value of these experiments, was shown admirably under question 1700. But the point here is, the "convenient doctrine" that only physiological workers can judge of the value of their work, even in its boasted results.

No other scientific profession would make such an acknowledgment. Nobody outside the profession can judge its value. Hence, in reference to the authorities of Dublin University, who objected to painful experiments, Mr. Huxley observed, as a question: "4846. So that here we have *an example* of *what inspection* comes to, *even* when carried out *by a highly educated body of men*, who do NOT HAPPEN TO BE PHYSIOLOGISTS?—Yes."

As none can understand these matters but those "who happen to be physiologists," or call themselves such, and of whose fitness there is no criterion, so it is assumed in the sixth place:—

6. That not even physiologists are competent to inspect or judge of each others' experiments. One being asked, respecting the propriety of certain methods pursued by a foreign master, modestly observed that it was not right for one to "sit in judgment" on the proceedings of another (2405).

Accordingly, physiologists themselves do not venture to judge one another as to the *propriety* or method of experimenting: they do, indeed, quarrel over the "inference drawn from" or theories "based" on "results;" and one man feels

exalted in disproving the *theories* of some other eminent *practical* inquirer; but the *morality* of the case—whether some cruel experiment was really cruel, or only "perfectly innocent," because "necessary" to carry out the investigator's hobby, or purpose—all *that* is outside anybody's province, except that of the operator's own conscience.

Thus when Mr. Forster, who as representing the Government, inquired whether Dr. Sanderson had conveyed the official instructions to those whom he, as presiding over the medical department, employed to experiment for the public health, he received in reply a rebuff, to the effect that one physiologist, however "highly elevated," would scarcely venture to give direction to another.

The two following questions and answers, between Mr. Forster and Dr. Klein, illustrate this individual autocracy:—

"3649. Would you inform him [Dr. Simon] beforehand of what you were going to do in the way of vivisection?—No. I think Mr. Simon *knows too much himself* about scientific investigation *to interfere with an investigator.*

"3650. I want to know whether he has ever told you, as a general *rule, for your guidance,* that, in any severely painful operation, chloroform is to be administered for any experiment connected with the Privy Council?—*The only direction* which Mr. Simon gives is, that *he gives the thema* to do this and that investigation; but *to say how* we are to do it, *or to criticise* the results we obtain, *I do not think he ever undertakes.*"

The greatest expert in "physiological matters" must never "interfere with an investigator"—"to say *how* we are to do it," is an offence against the sacred majesty of science.

They, therefore, are deplorably mistaken who presume to think that even expert physiologists are "competent" judges of some investigating process, in which "original research"

is the "purpose" and justification; only the special investigator himself can see "the drift."

This is clearly enunciated in Mr. Huxley's question:—"3323. And I presume that, *as a man of science*, knowing what investigation is, and being well aware that, *even among men of science, very few*, except *those* who are engaged *in a special* inquiry, are *really competent* to judge of its value, you would *view, with some trepidation*, inspection even by *competent* men of science?—I should."

This was Dr. Ferrier's answer, and it is possible that some of his experiments might awaken as much scientific surprise as public reprobation.

7. We come to the absolute autocracy of each individual operator. He is his own inspector; answerable only to his own conscience, with which sacred authority, even in a matter of dealing with creatures who may be said to have a claim on public protection, it is a sin and usurpation to interfere.

That there *is* a sphere of conscience, as between man and God, which no human legislation may invade, but which it should protect, is not denied; but the doubt which may be *conscientiously* entertained by the generality of mankind is, whether society may not interfere between a man and his treatment of animals, and whether even this interference may not be in the name of that God, who placed animals as much under our protection as control, for legitimate use, and not for capricious torture, under any pretence whatever.

This assumed *irresponsibility* to any authority but private conscience, in experimenting processes, leads logically to the next demand of the practising physiologists, that the public should stand aside and leave them to their own will.

8. *The public has nothing to do with it;* does not and cannot understand the matter, and its interference or

"outcry" or "furor" is impertinent "barbarism" (585). Respecting painful experiments for demonstration, or in the class-room, and therefore *a fortiori* for such experiments for "original research" in the laboratory, it is tenaciously held that the public has no right, and no capacity to judge. Thus respecting painful demonstrations, Dr. Purser is asked and answers as follows:—"4800. My view is that the matter should be left to the judgment of the professor (4801)—ENTIRELY?—*Entirely.*" "4802.* And that, *if the sentiment of humanity*, on *the part* of *the public interfered*, it would *be an undue interference?*—I think *it would* be an undue interference. I do *not* consider the public a *good* judge of such matters."

How thoroughly incompetent the public is to judge even of the *benefits* conferred by experimenting, and how utterly unworthy of regard "any general sentiment of humanity" is, when criticising or presuming to regulate these scientific processes, is again expressed:—"4805. In short, you give it as your opinion that your *own discretion* should be *the only rule*, and that any *general sentiment of humanity*, in the minds of the public at large, should be disregarded?—As I said before, I do *not consider that the public is a fit judge* of these things. The public, in the first place, do not know the *way* in which these experiments are performed; and in the next place, they do not know the *object* for which they are performed, and the *value* of the instruction to be gained from them."

In reply to the reiterated question, "Whether you contend that, in this matter, your own judgment, and *that alone*, should be the guide, and *that any* sentiment of humanity on the part of the public SHOULD BE EXCLUDED?" Dr. Purser said:—"I can *conceive* a case in which a professor would be extremely ill-judged, and perform experiments which were

* *This number is misprinted in the book*, 4002.

not necessary, and perform them in a way to inflict more suffering than necessary; "but *I do not think it is the* PUBLIC that SHOULD PREVENT him" (4806). 4808. "I consider that it would be a very unsuitable subject for legislation; 4809, Altogether?—Altogether. 4810. In short, you are so convinced of the good *judgment of the teacher* that you think *it should be superior to every other consideration?* —I think the teachers of physiology are the most competent judges."

As to whether painful experiments may be repeated, when it is supposed the results are "established," Dr. Rutherford, being asked about legislation providing "that some person or persons should give an opinion, as to *when a repetition was*, or was not, considered *permissible*?" (2947) answered: "I think you would involve the subject in endless difficulty, unless you left it to the *discretion of the person licensed* that he should do this or that." "I do not see how it could be done without really stopping physiological research" (2948).

"In fact," observes Dr. Sharpey, "it would be an act of *barbarism* at the *instigation* of certain people, in order to prevent what *they* conceive to be barbarity" (585).

Every effort was made, by coaching and coaxing, to induce the leaders to surrender to "the powers that be," and admit the propriety of some restraint; but this was often sturdily resented, and only grudgingly admitted, as a dangerous surrender of their rights.

A complete autocracy was claimed by the chief ministers at the shrine of physiology, and by some few others who worshipped before the dripping altar.

Thus, to the abolition of the claims of animals, by the right of experimenting, as far as necessary for their purposes, is added, the *abolition of the rights of the public*, or of Society in its aggregate capacity, to rule over the conduct of its members, who may offend or outrage public feeling. Not only is the medical profession to sit at the feet of the

practising physiologists, and learn from them how to make an "inexact and empirical" practice "more precise" and "purely scientific," but all who indulge in "weak sentiment" of mere "humanitarians," are warned to stop all "outcry" and "*furor*" (2898), and recognise in the new race of experimenting philosophers, a priesthood too "highly elevated" (5483) for common criticism, or heretical "lay" judgment.

To object to any experiment for its alleged "barbarity," was branded as a return to "barbarism" (585), from which calamity the licensing resort has saved us.

All that the *public has to do* with the matter *is to find institutions* in which the practising physiologist may carry on operations, which require appliances that are beyond his private means; and *all that the Legislature is bound to do* in the matter *is, to protect the physiologist* in his practices, to the extent dictated by his scientific purposes—according to his own conscience, of which it is presumption for "outsiders" to form a moral judgment.

All "weak sentiment," the "general sentiment of humanity," "the moral sense of the community," and "the rights of the public, if the public have any rights," are to be surrendered to the exigencies of a science, of whose value only its practising professors can judge.

The professed object of the Royal Commission was to prepare the way for "a Legislature desirous of giving effect to the moral sense of the community at large" (Report xvii.). The object of the special physiologists was to get into a position to bid defiance to popular opinion; and their expressions showed contempt for "the moral sense of the community at large;" and the result is, for the present, to screen the experimenters from public observation and influence.

The following question and answer indicate what has been carefully avoided—to Dr. Rolleston:—"1291. Would you be kind enough to tell us what you think is the *prac-*

tical object at which we [Commissioners] should aim?—To put it in a very general way, I should think the *practical object* was, *to put into a stable and permanent form the present wholesome* condition of *public opinion* upon the subject."

The result has been "to put into a stable"—it is to be hoped not a "permanent form"—the aspirations of physiology to be uncontrolled by public opinion which, though temporarily asphyxiated, may be resuscitated by the "experiment" of ventilating the whole question, as here attempted.

CHAPTER XII.

THE GENERAL UNCERTAINTY OF THE PHYSIOLOGICAL POSITION; AND THE OVERTHROW OF ITS CLAIMS.

FROM the dream of being specially "face to face with facts" in physiological speculations, we wake up to find that we have been dealing with fictions created by "the scientific use of the imagination." Most of the terms and phrases are indefinite:—

1. We have no clear knowledge as to what is an "experiment;" sometimes the ordinary treatment of patients by medical men is described as consisting of "experiments;" the Zoological Gardens with its lodgments of animals is confessed to be "pathological experiment" on "a gigantic" scale (2011); so that the "experimental method," applauded as the speciality of professed physiologists, is no speciality, and "experiments" are left an open question.

2. We are told to distinguish between a "purely scientific experiment" and "a pathological experiment" (5320) which, by this distinction, is not regarded as "scientific," though "pathological experiments" constitute a great proportion of those done by "professed physiologists."

3. We are left at a loss to know what is "*original* research" or "original investigation" and "scientific investigation," sometimes distinguished from "demonstration;" which distinction again is dismissed as a "shadowy line;" although some "reports" or "records" describe the experiments as "for research," "original research," for "scientific" purposes, on the one hand; for "teaching purposes," "for demonstra-

tion," on the other hand. [See Returns, Appendix to the Commissioners' Report and Evidence.]

Upon this eliminated or uncertain distinction is based the discussion—whether painful experiments should be permitted in "demonstration"—for teaching, or only for "original research."

4. What *is* a "*discovery?*" is abandoned as an insoluble problem or an obsolete inquiry, and all is wrapped up in "investigation," which, according to Dr. Burdon-Sanderson, includes examining the truth of old discoveries and facts in general, without the embarrassing and interminable question of "What *is* a new discovery" or "discovery" at all? Yet the main defence of the physiological system of experimenting rests on the claim to new discoveries made by it; as in the historical, if not mythical, cases of Harvey, Bell, and Hunter, whose achievements, whether through experimentation or through observation—that is, by artificial or natural processes—we are forbidden by physiological leaders to call "discoveries."

5. The *living* functions, or *life itself*, as the special field and subject matter of physiology, called in this relation biology, is still a mystery; and we are told that no one knows what life is, and that therefore "a live animal" cannot be defined, for the purposes either of law or science (3048-9, 3113).

6. An "important" object, or even a "scientific purpose" and "result" are equally undefined; and what is apparently, to some not bad judges, a trivial and an unphysiological process, is to others, also considered not bad judges, apparently most important and scientific; and is finally left, as a matter of fairness to the individual workers, who—as Mr. Huxley admitted, gravely, respecting Paul Bert's "curarised dog" experiment—may, in justice, be admitted

to have a right to consider the results of their own work important and beneficial (4164).

7. All the direct evidence of the beneficial results of "pathological"—*not* "*scientific*"—experiments, made under Mr. Simon by Drs. Sanderson, Klein, Foster, and Brunton, for the Government, was *given by the workers themselves.*

8. The cholera experiments were second-hand, copied from Foreign "original research," and they "proved" "by experiment" what had been "observed" at home without experiment.

9. *Fatal results of the licensing scheme.*

The method of licensing now adopted gives up all the glory of former alleged experimental discoveries, which were the productions of *free* investigation, open to private individuals, by whom, and not in schools or in licensed bodies, discoveries are generally made (2567-9).

The same method, by its *secrecy*, withdraws the matter from public observation, and, while professedly founded on a desire to satisfy the public mind, is really a fraud upon public opinion, like the anæsthesia apology and the curari deception.

It is, moreover, *a concession* that the business *needs secrecy*, and could not live in the light of publicity.

So the method has three effects: it excludes the public, and works in the dark, contrary to the rights of the nation and the open character of English proceedings; while by a *free license of action, within* the discretion of a privileged few, it condones the cruelty, and by excluding that personal method of free inquiry, which is the fosterer of discoveries, it destroys the utility, on the ground of which systematic physiological experimenting is defended.

CHAPTER XIII.

SUMMARY OF THE MAIN LINE OF ARGUMENT, AS THE POINTS ON WHICH THE WHOLE QUESTION IS TO BE DECIDED.

I. *Its failure as a question of pure science and logic.*

The *raison d'etre* of experimenting physiology is, that by placing medicine on a scientific basis, a clear light is given to the physician, for the better, surer, and safer proceeding, in assuaging the sufferings of mankind. This claim of teaching the medical adviser and practitioner to know exactly what he is about, and what to do, is distinctly set aside by a logical examination of the scientific claims set forth in defence; and it is proved, from the nature of the case, and from the admissions of leading advocates, that "the lamp put into the hands of the physician studying disease," is a "will o' the wisp," affording no certain guidance, no clear and steady light.

Practical physiology, so-called, is proved to be *sciolism, and not science*—it is pretension sitting in the chair of medical science, offering impossible knowledge, obtained by illogical processes from impossible sources, and mocking the medical profession, and through it mankind in general, with a delusive hope of a scientific certainty to be conferred on a subject, which, by its very nature, renders experience, observation, and *discrimination* a necessity, and renders a barrel-organ precision impossible of application.

The only scientific certainty which characterises experimenting physiology is its *uncertainty*, as the only moral certainty belonging to it is its cruelty.

II. *The failure as to historical questions of previous discoveries, set down to the modern system of exclusive professional experimentation.*

We have shown, both from their own canons of criticism and from rational considerations applicable to the case, that this *retreat into history* was a resort to uncertain and questioned "facts;" which facts, as relating to others, *not* engaged systematically in exclusive experimentation, even if "established," would be irrelevant; and which "facts," as "*discoveries*" are decried, and given up as undefinable and unprovable, by those who *appropriate* them to modern physiological experimentation; and which are so appropriated, in order to hide or make up for the barrenness of the system, by claiming fruits which never grew on that withered tree.

III. *Its practical failure as to its own professed work.*

This has been made manifest in a special *school examination*, and in many incidental illustrations. It is occupied "mainly *in revising*" its own conclusions; that is, in *abandoning* them; in "converting into a certainty," about dogs and cats, a *few* points observed or "suspected" about men; and, after all its experiments on animals, "leaves it to" the physician "to experiment on man," and test one "empirical" process by another.

It learns nothing and teaches nothing; but is always on the verge of an idea, "getting more precise knowledge" of some imaginary *contagium* with "its identification outside the body," or trying the value of "choleraic discharges," to ascertain that which Foreign experiments had shown, and what our "Home-bred" physicians had "suspected," and what common-sense would teach even "lay" people. Failing in any definite discovery,—that can be securely relied upon, for man or animal,—it falls into hysterical assurances of what, in some "not distant future," will be accomplished

by the method that has failed in the past, and draws all its credentials from its future performances.

IV. *Its moral failure.*

Its apology of insensibility produced in its victims by anæsthetics is a *double* moral failure,—that of covering cruelty by deception; and so adding this deception to the original offence.

To make its "horrible things" "very innocent," it hides one fault by another, and becomes thereby guilty of both.

It is the only professed science that lives on excuses, instead of on its own propriety. *Astronomy has not to apologise for itself*, neither has chemistry—*only physiology; it* is not trusted, but is manacled; its tendencies are dangerous. Its excuses are its worst faults; as condoning, and striving to conceal, those sins for which the excuses are the apology or pretext.

The plea of anæsthesia is practically a mask to hide cruelty.

V. *Its Social Failure, combined with its Moral Failure.*

In this it drops the mask assumed in anæsthetics, and turns bold in assumption, after being humble in exculpation: it justifies what it has vainly apologised for, and defiantly says in effect to "Society:"—"We have a right to inflict whatever suffering we consider necessary, and you know nothing about it, but are bound to protect us."

This is a repeal of the rights of Society, in order to justify an infringement on the claims of animals, by excluding the influence of "any general sentiment of humanity" "on the part of the public," as "an undue interference," with a science which professes to give certainty, and to serve humanity, by a method whose two specialities are uncertainty and cruelty.

THE GENERAL MORAL PRINCIPLE assumed through the whole, and especially in the claim for the protection of the

operator, in obtaining his "results," such as they are, at the expense of any extent of animal suffering, and the exclusive authority to decide what is "lawful and right," by what is necessary for his purpose, is well, though unintentionally, described by Mr. Dickens, in the following:—

"Still, I do mean to say, and to say distinctly, that it is the unvariable practice of many mighty philosophers, in carrying out their theories, to evince great wisdom and foresight in providing against every possible contingency which can be at all supposed to affect themselves.

"Thus, to do a great right, you may do a little wrong; and you may take any means which the end to be attained will justify; the amount of the right, or the amount of the wrong, or indeed the distinction between the two, being left entirely to the philosopher concerned, to be settled and determined by his clear, comprehensive, and impartial view of his own particular case."—(*Oliver Twist*, ch. xii., p. 53.)

PAINFUL EXPERIMENTS

ON

LIVING ANIMALS,

SCIENTIFICALLY AND ETHICALLY CONSIDERED.

BY

ABIATHAR WALL,

Licentiate of the Royal College of Physicians, Edinburgh; Member of the Royal College of Surgeons, England, &c.

"The proper study of mankind is man."

INTRODUCTION.

THE cultivation of physiology has for its object the discovery of those laws which govern and maintain the healthy functions of every part of the organised body.

Its importance in relation to medicine and surgery is such that any attempt to trammel its progress and development would be fraught with disaster to the best interests of the human race; and it is consequently asserted by many that the discontinuance of vivisection experiments would have this effect. They affirm that this method of experiment has been the means of many important discoveries; that nature presents so few suitable cases, and that these occur at such long intervals, that to wait for this species of investigation would be to retard, for long periods, the advancement of the science; that the infliction of pain upon the lower animals is perfectly justifiable, and is not in the least immoral, provided it be perpetrated in the belief that good will accrue from it; that the administration of anæsthetics, in most cases, renders the experiment painless, and therefore removes any objection that can be made upon the score of cruelty.

On the other hand, those who object to this form of experiment assert that vivisection never has been the means of discovery, and, from its nature, never can be; that the normal phenomena of nature and its aberrations are ever present, and amply afford every requisite for the due progression of human knowledge; that vivisection, being a fallacious method of research, has retarded rather than hastened physiological discovery, and has, moreover, been the cause of the introduction of many serious errors into practice; that it is indefensible on moral grounds, the plea that "the end justifies the means" being opposed to all

higher considerations; that the administration of anæsthetics to destroy pain is inadmissible in most cases, and that when they are administered, their effect can only be kept up for a short time, thereby leaving the animal exposed to every variety of agony for a longer or a shorter period, according to the requirements of the experiment.

Such is a fair statement of the question at issue, and its elucidation will tend to show what are the legitimate modes of research necessary for the development of a beautiful science, such as physiology undoubtedly is. It is self-evident that if legitimate and truly philosophic means be not employed its professors must be for ever going round and round the truth, but never arriving at it. Such has been the case with other sciences, until delivered from the bondage that held them in fetters of iron; then, and then only, did they spring into the light of day, free and untrammelled, shedding upon the world that blaze of happiness and prosperity which dispersed the ignorance and gloom of the dark ages. Physiology would have advanced much more rapidly had its earlier professors seen the necessity of cultivating the collateral sciences, more especially comparative anatomy, instead of attempting to explain its phenomena by a single science. Haller was the first to insist upon a proper study of anatomy; and Laurence justly observed that, "The extensive examination of various structures is not only a necessary groundwork for the edifice of general physiology, but it has thrown great light on the organization and *functions* of the human frame." "Whoever will reflect on our present knowledge of the digestive, respiratory, or other processes of man, and will review the successive stages of its progress, will find that *comparative anatomy* has rendered us the most vital assistance."

On taking a survey of creation, with its teeming myriads of living and animated beings, one sees the Sovereign-Architect of the universe distributing to all the elements of life and activity—giving to some a less power of motion, to

others more; so that, formed all upon one model, they seem only the infinitely varied but gradual shades of the same form; never passing abruptly from one to the other, but rising or falling by gentle gradations, covering the interval that separates two different beings with many species that serve as a transition from one to the other, and which present a continuous series of advancement or degradation; organization being constantly simplified in descending from man to the inferior creatures, but rising in complexity in re-ascending from those animals to man, who is the most complex being in nature, and was justly considered by ancient philosophy as the masterpiece of the Creator.

Many of the errors, both of primitive and modern physiology, must therefore be attributed to the want of a proper appreciation and knowledge of the structures of the infinite number of species inhabiting the earth; but as our anatomical knowledge advances, ignorance recedes and new physiological laws become established which point out with certainty the modes of functional activity displayed by the various organs.

Could any use be ascribed to the Thymus gland previous to the knowledge that it belongs, without exception, to all animals breathing by lungs, and to no others? This fact of comparative anatomy throws some light upon the obscure knowledge we possess of the functions of this gland. We are led to examine it in various animals; we ascertain that in hybernating species it becomes stored with fat just before the period of torpor; we see its large size in the human fœtus, and its gradual dwindling in infancy. From all these simple observations on the dead body a vast amount of practical information can be derived, and it is to be regretted that this science was not more assiduously cultivated during the infancy of physiology, for its study would, in all probability, have prevented that growing love for vivisection experiments which has now become such a grave and serious evil.

The rapid progress of chemistry and its application to physiology has been productive of incalculable benefit. Since the discovery of oxygen, medicine and the sister sciences have received an impetus such as they never before had; and indeed when we scan the history of the advancement of physiology, we are forcibly reminded at every point that all the great truths are due to the collateral sciences and not to vivisection experiments. Narrow, cramped views of physiology, and a want of a proper appreciation of ethical laws, have led men into error, so that nothing seems at last easier to say than that "here are animals for our use, let us discover the laws of our being by subjecting them to such experiments as may seem necessary," ignoring the fact that these experiments mean the substitution of disease for health, and that disease is so common amongst men that it affords ample bases for observation; forgetting that the different constitutions of animals admit, in experiment, of nothing more than an approximation, at most, to natural laws; and refusing to believe that the torture of the lower creation is an offence that morality revolts against, even though the suffering be inflicted with the intention of doing good to humanity.

CHAPTER I.

It is advisable to enumerate here some of those fallacies which fundamentally vitiate vivisection experiments, inasmuch as, when the contradictory views of physiologists are discussed, we shall be prepared to assign a correct cause for their diversities of opinion.

A well-known French physiologist laid down the rule, that "Success in physiological experiments demands that account should be taken of the particular conditions presented by the animal that is operated on." He then goes on to say, "I have invariably pointed out to you the examples that confirm this opinion. The conditions of health or of illness, of robustness or delicacy, of fasting or feasting, of hybernation or of vital activity in animals, exercise a *preponderating influence* on the results of the trial."

At the outset, then, we have vivisectors themselves ackowledging that numerous elements of fallacy may creep into their experiments and vitiate the results; but considering the absolute impossibility of accurately testing the state of health of an animal at any given moment, it would appear that the gauging of these "particular conditions" is somewhat more difficult than vivisectors care to admit.

But, irrespective of the state of health of an animal, there arise innumerable sources of fallacy during the progress of the experiment, some of the most important of which require to be pointed out. First, the administration of chloroform, whenever attempted, is certain to throw the system off its equilibrium. This substance is absorbed into the blood, and conveyed to the nerve centres, upon which it produces very marked effects, particularly upon the pneumogastric and sympathetic nerves and the medulla oblongata. That the poison has an injurious effect upon the nerve centres is

manifest from its mode of operation and from the vomiting it produces; whilst we have, in the disturbance of the muscular, respiratory, circulatory, and digestive systems, and in the cessation of reflex actions, evidence that the whole of the cerebro-spinal and the sympathetic systems are involved. As a necessary consequence of this, the various functions of the body are either stimulated or retarded; secretion goes on languidly, or is perverted, as is essentially the case with that of the liver; the stomach takes on a function vicarious to that of the lungs, and endeavours to eliminate the poison; a considerable fall of temperature takes place, which is partly due to the depressing influence on the circulation, as well as to its direct action upon the blood corpuscles.

Death may result from the chloroform giving the blood such a quality that the pulmonary capillaries refuse to give passage to it (the blood being found to be fluid after death, and the corpuscles wrinkled at their edges), or by acting on the medulla in such a way as to stop the reflex act of inspiration. Again, death may result from paralysis of the sympathetic ganglia of the heart; or, lastly, by the sudden annihilation of the life of the brain and spinal cord.

From this it is evident that the loss of balance of function, and the vitiation of secretion throws the whole system into a morbid state; and whilst it lasts, any experiments that may be performed are thoroughly vitiated and are practically worthless. These facts are so well known that, in the Vivisection Act, a special clause is inserted to the effect, that licenses may be obtained for the performance of experiments upon animals without anæsthetics, provided it can be shown that their administration would mar the result of the operation! Most of the experiments on living animals have reference either to the nervous or digestive systems, both of which present most palpable evidence of disturbance under chloroform, so that there actually occur only a very small number of cases in which vivisectors would consider anæs-

thetics to be admissible; perhaps they can only be exhibited when an operation (such as the establishment of a gastric fistula) has to be performed prior to the experiment itself; but in all other cases their employment can but conduce to error.

Another drug, curari, which is frequently used in the physiological laboratory, is hyperæsthetic in its action, that is to say, it increases the ordinary sensibility of the body, but at the same time it paralyses the motor apparatus. Claude Bernard says of it, in the *Revue Scientifique* (vol. vi., p. 591):—"Curari renders all movement impossible, but it does not hinder the animal from suffering and from being conscious of pain."

Perhaps the best accounts we have of the action of this substance are derived from the personal experiences of human beings who have had overdoses administered to them. They state that they were perfectly conscious of all that was passing around them, but were entirely bereft of the power of making the slightest movement. In the *Revue des Deux Mondes*, for September, 1864, is an article from the pen of Claude Bernard, in which he gives the following graphic description of death from curari:—"We discover that this death which appears to steal on in so gentle a manner and so free from pain, is, on the contrary, attended by the most frightful sufferings that the imagination of man can conceive."

The perfect freedom from struggling, and the absence of the slightest movement in the curarised animal, would naturally lull the suspicions of a bystander to sleep—he would scarcely credit the terrible agony the animal was undergoing beneath the knife of the operator, an agony increased by reason of the heightened sensation imparted by the drug. All this time the subject has to be kept alive by artificial respiration; if this is not persevered in it would die, such is the depressed vitality produced by the curari—indeed, its life literally hangs by a thread. But here we

have a condition presented by the animal operated on which must, according to the cardinal rules laid down by eminent vivisectors, "Exercise a preponderating influence on the results of the trial." We are told that the animal must be in the best possible state of health, whereas, as a matter of fact, we have to draw our inferences from an animal at the point of death. The poisoning a dog with curari, mutilating him with the knife or caustics or acids, and generally upsetting by these means the harmonious working together of the various organic functions, thus bringing him into the actual throes of the death agony, hardly seems a philosophic method of ascertaining the working of these functions. One would scarcely attempt to determine the normal composition of the blood in a man just dying from some fever, yet normal functions are attempted to be discovered in a dog dying from curari!

The mere fact of confining a healthy animal in one position is sufficient to derange circulation and respiration, and through these the other functions of the body; and, as everyone knows that an animal cramped up in an irksome position will struggle for hours until it is thoroughly exhausted, it is evident that if the experimenter waits until this happens, he will have anything but a healthy subject to work upon. Practical works on physiology lay it down, as a cardinal rule, that it is necessary to see that the normal state is recovered before commencing the operation, and that this must be particularly attended to if the pulse, blood pressure, respiration or temperature have to be studied. But when it is considered (as Horvath ascertained) that the mere binding of an animal causes a gradual fall of temperature, sometimes as much as 3° F., and that the struggling and fear induced by this (to it) novel method of restraint may still further reduce it, it is impossible not to see that we have introduced so many disturbing elements which cannot fail to render any result nugatory.

The identity of functions that vivisectors seek to establish

between the lower animals and man is another fertile cause of physiological error. It is a well-known fact that, apart from anatomical differences, most animals present such vast physiological differences from man as almost to make one wonder why these kinds of experiments should ever be persevered in! Vivisectors are agreed that the selection of an animal for a particular experiment is one of the most delicate in technical physiology, and assert that such knowledge can only be acquired by prolonged experience. If, then, the inexperienced operator seeks to experiment, the chances are that, for a long time to come, the results he obtains by the sacrifice of life will simply be worthless, except in giving to him the necessary experience in the choice of animals.

If we reflect upon the peculiar differences between man and the lower animals, we shall come to some important conclusions as to the value of vivisection experiments.

In the first place, has it been explained how it is that horses can take large quantities of antimony; dogs, mercury; goats, tobacco; mice, hemlock; and rabbits, belladonna, with perfect impunity, whilst small quantities of most of these substances would be sufficient to kill a man? Again, the natural aperient of the dog, couch-grass, has not a similar effect upon man; whereas parsley, an innocent herb to the human being, is a deadly poison to parrots. Thirdly, how is it that most, if not all, animals can eat food in such a state of putrescence without any ill effects, as would be sufficient to cause symptoms of poisoning in man?

These points, a few out of many such, cannot be disputed, and they amply demonstrate that the digestive systems of man and brute present great differences; and yet the dog is chosen for ascertaining the effect of various drugs upon man! If the gastric juice, bile, and pancreatic fluid have, in the lower animals, a peculiar power of digesting and assimilating substances which the fluids of the human body cannot, it is plain that there must be some peculiar difference

between those of each class, and that this difference of composition may probably be accompanied by variation of anatomical structure. What we infer from these observations is proved to be true by chemical analysis; the gastric juice of the dog, for example, containing nearly six times as much pepsin as that of man, and nearly twice as much free hydrochloric acid.

There can hardly be the slightest excuse for the attempt to establish such identities of function solely from an idea of analogy. We might as well claim the results of an experiment on the digestive organs of a crustacean to represent identical results with those obtained from a human being, as to believe that the different digestive juices of a dog can have the same action on drugs as have the digestive fluids of man!

With reference to substances which get into the circulation by absorption, as in the case of bites from poisonous animals, there is generally to be found a marked difference in their action on man as compared with the lower creation. This difference of action is especially noticeable in those animals which form the principal victims of the physiologist. To some the substance injected is a powerful poison, to others it simply causes temporary inconvenience, whilst to a third group it is perfectly innocuous.

Occasionally a drug, whose action is, as a rule, uniform on man, in a few instances, departs from this regularity of action, and produces a perfectly different train of symptoms, constituting in the subject of this new action what is called an "idiosyncrasy." As an example of this, the action of opium may be cited. It is, as a rule, sedative; but in a very few instances it is found to possess no sedative powers, but acts as a smart aperient. Now, the plea of idiosyncrasy cannot be set up by physiologists in those cases where the animal is differently affected than man; this diversity of action between the two holds good for the whole of each class, whilst the few departures from this law are solely

the idiosyncrasies. Vivisectors, therefore, can but acknowledge what their own experiments must forcibly teach them that animals of a different structure from man are not affected by drugs in the same way as the latter.

Dr. Livingstone's account of the Tse-tse fly, found in Central Africa, well illustrates this:—"The bite of this poisonous insect is certain death to the *ox*, *horse*, or *dog*. The *mule*, *ass*, and *goat* enjoy the same immunity from the Tse-tse as man and the game. Many large tribes on the Zambesi can keep no domestic animals, except the goat, in consequence of the scourge existing in their country. Our children were bitten, yet suffered no harm; and we saw around us numbers of zebras, buffaloes, pigs, pallahs, and other antelopes feeding quietly in the very habitat of the fly. There is not so much difference in the nature of the horse and zebra, the buffalo and ox, the sheep and antelope, as to afford any satisfactory explanation of the phenomenon."

It seems strange that two members of the same family, *though of different genera* (*e.g.*, the horse and zebra), should be so unequally affected by the same poison—the one being certain to die, the other escaping scatheless; but it serves to show that the difference of structure in genera of the same family implies a difference of physiological action. Hence this affords one more illustration of the danger of generalising from experiments made on animals, even if claiming a sort of kinship. If then animals, presenting but slight differences between each other, behave so differently under the action of a poison, what must the gap be between them and man?

In the case of the Tse-tse poison, man escapes its deadly effects, whilst the horse or dog does not. Is it not patent that this divergence of action must be due to some great physiological and anatomical demarcation between them? Had the action of the Tse-tse poison been unknown in Africa, and had it been sent home to be experimented on with the result of fully establishing its lethal effect upon

the horse and dog, we should have been told that it was therefore a poison to man! This would be an utterly wrong conclusion, but would amply demonstrate the folly of arguing from the lower animals to human beings.

One of the most fertile sources of derangement of function is *pain*. No operation can be performed without causing it; if chloroform be given, it can only deprive the animal of sensation for a short time; and as few observations can be made until it returns again to consciousness, it is obvious that pain must be experienced by every animal operated upon. Chloroform is the *bete noir* of the vivisector, since it introduces fallacies and obscures the voluntary motions; hence he prefers the animal to retain consciousness and suffer agony. Anything producing pain must necessarily be accompanied by nervous excitement, resulting in an increase of temperature; and this influence of the nervous system alters, in some way, the chemical processes from which heat is commonly generated — that is to say, it modifies all the functions of organic life. Thus, during the continuance of severe pain, most of the organs of the body have their functions more or less accelerated or retarded, or even suppressed, and their secretions perverted from their normal standard. It may be laid down, as a general rule, that the "production of secretion is largely influenced by the condition of the nervous system." Whatever, within certain limits, excites the nerves of a gland, is followed by an increase in the quantity of its secretion. This is illustrated by the flow of tears and the increased discharge of saliva which often accompany a paroxysm of neuralgia in the fifth pair of nerves.

Through the nerves, various conditions of the mind also influence the secretions, not only in quantity but in quality. For example, milk acquires *poisonous* properties spontaneously, under certain circumstances. Dr. Krummacher relates (Taylor on Poisons) the case of a woman who

became violently excited over some domestic trouble, and in this state she suckled her child, with the effect of causing it to vomit. Some hours afterwards she again suckled it, when it was attacked with convulsions, and died. It appears, then, that mental emotions, whether the result of pain or the passions, does not tend to preserve the functions of the body in a healthy state, but, on the contrary, has the effect of vitiating them. In the painful experiments, performed to ascertain the effects of various drugs upon the secretions, these facts ought to have some weight, and deter the physiologist from proceeding with his useless task.

The instant that the brain becomes cognisant of pain, convulsive motions and spasms of the muscles supplied by the respiratory and facial nerves take place. Hence arise the convulsive sob, the cry of anguish, and that peculiar expression of the features so diagnostic of anguish.

In consequence of the very intimate nervous connection that exists between the brain and the stomach, the latter rapidly becomes deranged; the gastric juice is secreted in smaller quantities, and is vitiated in quality; the skin and kidneys have their secretions increased, as witnessed in the cold sweat of agony and involuntary micturition; the muscles lose their tonicity; the propelling power of the heart is reduced, and (as Müller points out) this disturbance of the heart proves that the wave of pain extends beyond the sphere of the cerebral nerves to the parts governed by the sympathetic. Every part of the body participates in this general abatement of vitality, though undoubtedly, in long continued, severe pain, this lowering of the vital functions affords some slight relief to the victim by blunting the sensibility of organs and the perceptive power of the brain. But, though made less poignant by long duration, yet pain still exists; it is only when it becomes so intense and the nervous influence is exhausted, that insensibility supervenes. When severe pain is suddenly inflicted, as by burns or operation, the first act of the recipient is an

energetic outburst, the whole body being thrown into a state of violent tension or exertion, the muscles becoming tremulous, and the whole system showing that it is labouring under the effect of great excitement. These intense spasmodic acts draw the regular supply of nerve-force from the organs, thereby seriously deranging their action. Following this intense and painful stimulation comes the prostration from the partial exhaustion of nerve-force. Pain, inflicted in any part of the body, is not therefore confined to one spot; but, by deranging the whole system, it induces painful feelings throughout, causing, on the mental side, intense emotion as the accompaniment of the physical condition.

Every physical pain, no matter how induced, has its action upon the mind, and the mental emotion may, by itself, be sufficient to cause death. In illustration of this may be mentioned the method adopted by Louis XIV., to convert the Reformed of the Cevennes. The attempt at conversion was effected by tickling the soles of their feet until they abjured their creed. Many died in the convulsions and immoderate laughter which it excited! This also demonstrates the effects of irritation of the extremities of nerves upon the functions of other nerves, either of the same or of a different order, by means of the nerve centres. There seems to be a well-defined gap between the long-sustained and intense agony of a vivisection experiment, and the natural pain of disease. In the latter, the sensations are more those of discomfort than actual pain, except in a very few cases; whilst, as the disease progresses, a toleration for pain is set up to a certain extent. In the case of vivisection, an experiment, lasting over some three or four days, would concentrate the pains of a long life-time into a short space of supremest agony. Nature is less cruel than man; she seldom or never causes, in disease, that horrible torture, the infliction of which seems to be arrogated by man as his special prerogative.

The conclusion to be drawn from the foregoing is that

pain, by deranging the healthy functions of an animal, renders futile any attempt to investigate, by vivisection, those functions. It would seem impossible to arrive at a knowledge of the normal processes of the body by inflicting pain, for the mere fact of opening a living body suffices to alter its condition, and thus make the object sought for elude our grasp.

The infliction of torture as a necessary accompaniment of investigation is evidently a direct contravention of nature's laws, and she shows this not merely by a retardation of secretion, but by such extreme perversion of the nutritive fluids, as to render the flesh of the animal poisonous. The following case, recorded by Baron Liebig, is to the point (Liebig's Letters on Chemistry, Appendix):—"A roe-buck was caught in a snare, and the abdomen and pelvis of the animal being enclosed in the cord, it must have died after a most agonising struggle. On eating its flesh, symptoms resembling those caused by the bites of rabid animals manifested themselves. The husband recovered after three months' illness; the wife died after lingering more than two years; the daughter, man-servant, and maid, who had eaten but little of the tortured animal, were soon cured." Dr. Röser, who attended these people, concludes his communication in these words:—"Many an animal is tortured to death in a most barbarous manner, like the roe-deer in the snare. Ought not then the medical police to be led, by cases like this, to adopt the strictest regulations to prevent the use, as food, of animals which have been in any way tortured to death, and to insure that the animals intended for food are not tortured before being slaughtered."

Seeing that one and all of these processes of investigation into the functions of living animals are fallacious, it is certain that nature herself has distinctly planted her barrier against what, *prima facie*, is a cruel method of discovery. Anything that disturbs that bodily state, termed "comfort," acts prejudicially upon the physical health; pain, immo-

bilisation, and the use of poisons so deteriorate the secretions as to produce the very reverse of what is essential to the experimental physiologist—a perfectly normal organism. The more we ascertain by observation the functions of the lower animals, the more are we led to believe that, to attempt to set up an identity between these and man's, and to assert that the result of an experiment upon an animal is equivalent to one performed on man, is an assertion overcharged with all the elements of unscientific absurdity. These barriers, and the impossibility of ever overcoming them, by reason of their being the immutable laws of nature, are the causes of the barren results of vivisection. This is why the late Claude Bernard exclaimed, "Without doubt our hands are empty now, but our mouths may be full of fair promises for the future."

CHAPTER II.

HAVING briefly reviewed some of the fundamental causes of error which beset experimental research into the functions of animals, and having demonstrated that no experiment can be performed without one or more of these sources of error being invariably present, we shall now be in a position to appreciate, at their right value, the labours (!) of eminent physiologists, whose incongruities and wrong interpretations of phenomena will form the subject of this chapter.

There, undoubtedly, do exist cases where vivisectors stumble upon fairly correct solutions; but when these cases do occur, it invariably happens that they merely amount to corroborations of theories previously deduced from clinical or pathological observations.

Physiologists have adopted the practice of establishing fistulæ in certain organs—as the liver, stomach, and pancreas—in order that the secretions proceeding from these may be collected and examined, and that their action upon food may be ascertained. With respect to the establishment of gastric fistulæ, for the purposes of experiment, it can safely be asserted that it has never been necessary; for all the important facts bearing on digestion have been collected by observation on persons afflicted with fistulous openings into the stomach or small intestines. Beaumont, Circand, Helin, Londe, Grünerwoldt, and others have carefully studied the subject in the numerous cases that came under their care, and all the facts we know have emanated from these, whilst the chemist has ably seconded their observations.

Schwann established biliary fistulæ in eighteen dogs, by forming an opening from the exterior of the body in com-

munication with the gall bladder, and removing a portion of the common bile duct. Of the eighteen animals thus operated upon, ten rapidly died from the effects of the operation, two recovered, and the remaining six died at various intervals. These latter six died from inanition; they lost weight, became emaciated, staggered in their walk, and suffered from extreme muscular debility. From all this Schwann concluded that the bile is indispensable to life! All this torture was apparently requisite to prove what any one, with a fair knowledge of comparative anatomy, would have regarded as a foregone conclusion. The liver is the largest viscus in man, and its presence is so constant amongst all vertebrata, as well as in the greater part of the invertebrata, that any doubt as to its secretion being indispensable seems absurd.

The cutting off of an important secretion from the intestine, thus preventing it from exercising its due functions of reducing the fatty particles of food and thereby deranging digestion, would necessarily produce emaciation, and ultimately death from starvation. It required no experiment to prove this. We might just as well remove the stomach from an animal, in order to ascertain whether the gastric juice was essential to life!

We are also informed by physiologists that the establishment of biliary fistulæ has been the means of discovering that the bile has the effect of reducing the fats and oils to a state favourable for absorption. This is undoubtedly true; but the experiments were, nevertheless, superfluous; for biliary fistulæ not unfrequently occur in the human subject, and therefore give all the requisites for observation without causing pain. Moreover, the ancients were accustomed to regard the biliary secretion as an animal soap, and they correctly assigned to it the function of emulsifying the fatty compounds; and in addition to this, the use of ox-gall in removing grease has been known to housewives from time immemorial. All the elements for reasoning correctly

from natural phenomena were at hand centuries ago; but men seemed to prefer the more tortuous method of experiment, rejecting Nature's proffer, and floundering on amidst a sea of accumulated error, until they set up theories untenable on pathological grounds and rejected by the dictates of reason. These constant errors have had the result of stimulating vivisectors to increased exertion to redeem their honour. Thousands of animals have been tortured to death without advancing physiological science one atom. The real progress is obtained by discoveries worked out independently of vivisection or of any cruel experiment whatever.

Another set of experiments have been performed to ascertain the uses of the hepatic artery by tying it. The experimenters asserted that they saw the secretion of the bile going on after the operation! Here must be an instance of ocular illusion or mental delusion! By intercepting the supply of arterial blood, the liver would be deprived of nourishment and remain inactive; for the blood of the portal vein would be unable to supply pabulum to its substance. Again, the tying of the portal vein affords no clue to the secretion of the bile; for although after ligature the secretion is seen to stop, yet the experiment which suspends the venous abdominal circulation is too speedily fatal to justify any conclusive inference. These operations, then, are useless; and it is on analogical proofs that the received hypothesis of the method of the secretion of bile is founded.

The theory, that the liver is the supplementary organ of the lungs, is one that has been mainly established by chemical science. The idea of this function of the liver *originated* by observing its enormous size, its existence in almost all animals, its receiving venous blood from all the organs of digestion, and the large quantity of blood carried into it compared with the small quantity of bile secreted. These observations led to the belief that it must possess other functions, the truth of which was proved by the dis-

covery of glycogen. This discovery is important, as it throws light upon many hitherto obscure pathological states, and, as is always the case when anything of moment is found out, it was not discovered by vivisection.

The experiments that have been made to ascertain the action of various drugs upon the biliary secretion are numerous; but the results obtained by different physiologists are very conflicting, and in no wise justify the large sacrifice of life involved in their deduction.

As the *Lancet* points out, with regard to Dr. Rutherford's experiments, that "It is impossible even to mention the various drugs which have been made the subject of experiment, which are at least fifty in number," only one substance, calomel, will be selected for the purpose of criticising the results obtained by its administration to dogs.

The researches of Professor Hughes Bennett on the action of calomel, published in the "British Association Reports" for 1868, showed that it *always diminished* the solid constituents of the bile, and, with one exception, the fluid portion also. Röhrig, in 1873, showed that, though calomel seldom recalled the biliary secretion after it had ceased, yet it *somewhat augmented* it when it was taking place slowly. At the very outset, two contradictory results are arrived at by experiment, and in order to ascertain accurately what the real action of calomel was, Dr. Rutherford instituted his series of experiments, which were published in 1875. Previous to this time, the whole medical profession held that calomel had the power of *increasing* the various secretions, notably that of the bile, and hence it was largely used in those affections of the liver where its secretion is deficient, and its exhibition in those cases was always attended with beneficial results. The preconceived theory of physicians was therefore adverse to that advanced by Hughes Bennett, and clinical observation was apparently defeated by vivisection, particularly when the result of Dr. Rutherford's experiments corroborated Hughes Bennett's deductions.

In examining Rutherford's first series of experiments, it is premised that they were all made on dogs who had previously fasted eighteen hours. After this prolonged abstention, the animal is paralysed by the injection of an aqueous solution of curari into the jugular vein; the windpipe is then opened and a tube inserted therein, for the purpose of keeping up artificial respiration; then the abdomen is opened, the stomach and duodenum are pushed aside, the free edge of the liver is raised, the common bile duct is dissected out, divided, and a glass tube inserted into it; the gall bladder must now be compressed so as to fill the tube with bile; and in order to prevent the return of the bile to the gall bladder, the cystic duct is clamped. All this having been accomplished, the wound in the abdomen is closed, and the animal wrapped up in cotton-wool, so as to restore it, if possible, to its normal temperature. This operation occupies about half an hour to perform; no chloroform or anæsthetic is given, the dog being merely curarised, and consequently more than ordinarily sensitive to the pain it is undergoing.

If any one has witnessed the symptoms of the passage of a gallstone down the bile duct, in the human being, he has seen some terrible agony; let him picture, then, a dog suffering more than this awful agony, often for eight hours, and he will have some faint conception of what vivisection is. But the poor animal is not allowed to remain quiescent in this condition; for the wound in the abdomen is opened two, three, or even four times during the course of the experiment, to inject the drug through an opening made into the duodenum. Each time the abdominal wound is closed again, and the dog wrapped up in cotton wool as before.

Can it be imagined that this dog is now in that healthy state which physiologists so insist upon as necessary for deducing accurate results? The animal has been starved, poisoned, paralysed, cut open, and its viscera handled and

disturbed; its normal healthy temperature has sunk several degrees; it cannot breathe except by an artificial process; its sensation is heightened, so that it suffers intolerable agony, and its whole system has received a severe shock. Well may Dr. Rutherford remark that this operation "is not so simple as it appears," and well may he lay down elaborate cautions as to its performance. He tells us that the abdominal viscera must be handled with great care, so as to avoid all dragging at the bile duct, otherwise the secretion of bile becomes so irregular as to *render the experiment useless;* the glass tube (in the bile duct) must be placed in such a position as will permit it to move with the diaphragm, but will prevent it twisting the duct which would impede the flow of bile. Artificial respiration must be maintained with great regularity, otherwise the biliary flow is rendered unequal by irregular diaphragmatic compression of the liver; and, moreover, if the respiration be deficient the secretion of bile is always diminished. Assuming it to be possible that all these precautions can be accurately carried out, the animal in its miserable, mutilated state would still defy the power of man to make correct observations upon its biliary secretion; for its functional activity is upset and altered, and its nervous system no longer acts normally.

With reference to the action of curari, it is stated that a dose, "just sufficient to produce paralysis, does not *apparently* affect the biliary secretion;" if too much curari be given, the heart's action becomes weak and irregular, and the secretion of bile diminishes. This effect of curari was ascertained by two experiments. In the first, secretion diminished until the middle of the third hour, when it began to increase somewhat; but at the end of the eighth hour it had diminished to less than was secreted at the end of the first. In the second experiment, secretion increased and diminished in a very fluctuating manner, which Dr. Rutherford explains by stating that it possibly might have

been due to the shifting of the glass tube. Here is therefore a source of error, making each experiment tell a different tale; but notwithstanding this, we have to believe that moderate doses of curari do not affect the biliary secretion! To those who are not satisfied with this dictum, based upon inconclusive experiments, curari may appear as another source of error when introduced into the system.

The introduction of a drug into the duodenum instead of into the mouth, thereby missing the action of the various secretions between the mouth and duodenum, lays the results of the experiments with calomel open to very serious objections; and in addition to this, the systemic disturbance induced by all this horrible proceeding, must mar the results. We know for a fact that any sudden fright, pain, or upset of functional equilibrium can so disturb the biliary secretion as to produce jaundice, an example of which is seen in the case of the abbé, in whom it was induced by a mad dog running against him. This jaundice may arise from the suppression of the functions of the liver, and it is more than probable that the pain and fright suffered by the dog has this effect to a greater or less extent. Hence an erratic or diminished flow of bile would result. All these sources of fallacy must have existed in the following experiments on the action of calomel.

The first experiment (experiment 30), made by injecting ten grains into the duodenum, resulted in *increased secretion* of bile, and on introducing a further dose a *still greater* flow took place. In the remaining three experiments that were made, the flow was *diminished*; hence it is concluded that calomel diminishes the biliary secretion. It is difficult to account for the remarkable variation between the first and the other experiments. Dr. Rutherford asks "whether this increase might not have taken place even if no calomel had been given?" Very possibly it might, but there are

no certain data to prove the truth of Rutherford's surmise. We may also equally well assume that the decrease of bile which occurred in the other experiments might have taken place independently of the administration of calomel. If fallacies are admitted to have an existence in one experiment, there can be no guarantee against their being present in subsequent ones performed under exactly similar conditions as the first. It is difficult to understand how, under these circumstances, any conclusion whatever could have been drawn, but the fact remains that these researches were supposed to have proved conclusively that calomel has no action whatever on the biliary secretion of dogs.

In 1877, Dr. Rutherford again returns to the charge, probably under the impression that the result of his previous experiment was erroneous, for he says that "possibly its (calomel's) non-action on the liver was due to an absence of bile from the intestinal canal." In this second series, calomel was mixed with bile and then injected into the duodenum.

In experiment 31, the first performed in this manner, the biliary secretion was *increased* after the administration of the first, second, and fourth doses, but we are told that the main result was *diminished* action. We are not told why calomel and bile should occasionally increase and occasionally diminish this secretion; to obtain a correct solution, one would imagine it to be necessary that the drug should produce either a uniform increase or a uniform diminution of bile. In the next experiment (31A) the biliary secretion was diminished, and hence it was concluded that calomel and bile powerfully stimulate the intestinal glands, but do not excite the liver. This conclusion deserves to be compared with that arrived at by Schiff, who states that the flow of bile from a biliary fistula is *much increased by the injection of bile into the small intestine* (Schiff, Pflüger's Archiv. III. (1870) 398). It appears then that Schiff, by injecting bile, managed to *increase* the activity of the liver,

whilst Rutherford, by doing the same thing with the addition of calomel, succeeded in *diminishing* it! Was it possible that the calomel neutralised the action of the injected bile? Or did any error creep into Schiff's or Rutherford's experiments? Some of the results of experiment 31 negative the conclusion that calomel can render either the bile or the secreting cells of the liver passive, hence we are left in a greater dilemma than before, not only with regard to the action of calomel, but in respect to the action of the bile. These two eminent physiologists have very much complicated matters by means of experimental inquiry. As these failures, or conclusions contrary to the received hypothesis, might have been due to the want of admixture of the gastric juice with the calomel, Rutherford investigated this point.

Mialhe, as long ago as 1840, ascribed the action of calomel on the liver to be due to its partial conversion into mercuric perchloride (corrosive sublimate), by means of the alkaline chlorides contained in the saliva and gastric juice. He was led to this belief in consequence of a case of poisoning which occurred in France, through the administration of some powders containing calomel mixed with ammonium chloride.

Dr. Rutherford, on the other hand, managed to partially convert calomel into corrosive sublimate by digesting it in water with hydrochloric acid (the acid of the gastric juice), and keeping it at the temperature of the stomach (100° F.).

Experiments were now made with mercuric perchloride; in the first, two-fifths of a grain, in all, of this substance were given in six successive doses, unmixed with bile. The results were negative, there being, however, a very slight increase of bile after the fourth dose. As the dog used was small and weak, Dr. Rutherford thought that the animal was to blame for his non-success, "for," says he, "cholagogues sometimes fail to stimulate the liver when the animal is in

this state;" or again, the negative results might have been due "to the depressing effect of the preliminary operation."

In experiment 33, and the succeeding ones, bile was added to the mercuric perchloride, which latter was then proved to be a *powerful hepatic stimulant.*

If this result be contrasted with that arrived at by Hughes Bennett, we find each the antithesis of the other. Hughes Bennett states that "corrosive sublimate, when given in small doses, gradually increased in strength, does *not augment* the biliary secretion; but that it diminishes it the moment the dose reaches a strength sufficient to deteriorate the general health."

It should be stated that five experiments were performed with mercuric perchloride, and in two of these calomel was added. In order to imitate as closely as possible the natural processes of the body, calomel was injected into the stomach instead of the duodenum, the dog being curarised. The result obtained was purely negative, but this need surprise no one, for the drug was found, unchanged, enveloped in the mucus of the stomach. This mucus accumulates so rapidly (we are told) in the stomach of a fasting dog as to hinder absorption. This fact was evidently known to the experimenters previous to the performance of the operation, hence the impossibility of obtaining a satisfactory result rendered the trial not only useless but purposeless. Moreover, it is known that when mechanical irritation of the stomach is carried beyond certain limits so as to produce pain, the secretion of the gastric juice, instead of being more abundant, diminishes or ceases entirely, and a ropy mucus is poured out instead, so that the probability is that the calomel, in the case under consideration, never had the opportunity of coming into contact with normal and healthy gastric juice. Have we not, then, in this proceeding, everything to constitute the "experimentum fallax?" The viscid saliva enveloping the calomel, its viscidity being due to the

prolonged fast of the dog; the ropy mucus thrown out instead of gastric juice, the alteration of this secretion resulting from the pain and irritation suffered; and the unnatural condition generally of the animal, one and all combine to vitiate any result. Further than this, the pain and irritation caused to the liver and its ducts must have had an effect upon its secretion, rendering its flow excessively erratic, at one time increasing or diminishing the quantity of solids, at another time the quantity of water. Dr. Rutherford informs us that he purposely "threw out of action the *bile-expelling* mechanism" so as to "deal with the *bile-secreting* apparatus only." The question is whether it is possible to do this without interfering in some way with the function of secretion? Regarding the nerve supply of the liver, we find representatives of the cerebral, spinal, and sympathetic systems governing its functions, whilst the gall-bladder is equally invested by branches of the hepatic plexus of nerves. Now it seems hardly possible that the irritation caused by division of the branches of these nerves, which happens when the common duct is cut across and the cystic duct clamped, would have no effect on the secretion of bile. We are aware of the fact that irritation of one of its nerves, the pneumogastric, so modifies the action of the hepatic secreting cells as to cause the production of sugar, "and which," says Dr. Harley, "we must look on as a result of *derangement or perversion of its functions.*" If it be argued that the nerve fibres divided are so extremely small and insignificant as to deserve no notice, it nevertheless cannot invalidate the fact that the irritation produced will travel from the circumference to the centre, and will be felt therefore throughout the whole of the nerve-system, and consequently have a prejudicial effect upon the bile secretion. Moreover, the presence of the glass canula in the bile duct during the whole of the experiment will tend to keep up and intensify nerve irritation. There is still another way whereby the flow of bile may be rendered

irregular, irrespective of any alteration in its secretion. The rough usage to which the common bile duct is exposed must set up an inflammatory process; and the inflamed mucous coat may so swell as to cause a narrowing of the passage, and therefore a diminished flow of bile. There seems, consequently, a strong presumption for believing that by throwing the bile-expelling mechanism out of gear, both secretion and expulsion of the bile are also thrown out of healthy action.

The general conclusion arrived at by Dr. Rutherford seems to be that calomel, *per se*, rather tends to diminish biliary secretion, but by its partial conversion into corrosive sublimate (whether by the alkaline chlorides of the digestive secretions, or by the hydrochloric acid of the gastric juice) it becomes a powerful hepatic stimulant. As previously pointed out, Mialhe had shown, in 1840, that the cholagogue action of calomel depended upon this change; but whether this is caused by an alkali or an acid, the fact remains that physicians rightly used this drug, and were aware of the necessity of its chemical change for its proper action on the liver. Fortunate, indeed, it is that medical practice is so little swayed by the results of vivisection; it might have been serious had physicians adopted the conclusion arrived at by Dr. Rutherford in his first series of experiments, for they would then have discarded one of the most useful drugs we possess for the treatment of liver affections. The number of actual and possible fallacies in these experiments, and the conflicting results obtained, notwithstanding the exceeding care taken, must shake anyone's confidence in the accuracy of experimental physiology. As long as pain exists, and as long as there are nerves to govern function and to transmit pain-waves, so long must there be functional disturbance, altering secretion both in quantity and quality; these are Nature's barriers and Nature's protests against scientific cruelty, and against the violation of the moral law.

ACTION OF CALOMEL ON THE BILIARY SECRETION.

Original Opinion—Bile increased.
Hughes Bennett—Bile diminished.
Röhrig—Bile somewhat increased.
Rutherford (1st Series)
Expt. 30—Bile increased.
31, 32, 33—Bile diminished.
Rutherford (2nd Series)
A—*Calomel with Bile.*
Expt. 31—Bile increased. Bile diminished.
31A—Bile diminished.

Rutherford (2nd Series contd.)
B—*Calomel partially converted into Mercuric Perchloride.**
a. Without Bile.
Expt. 32—Bile very slightly increased.
b. With Bile.
Expt. 33 *et seq.*—Bile much increased.
C—*Calomel with Gastric Juice.*
Expt. 38—Entirely negative.

* Compare these Results with Bennett's Experiments with Mercuric Perchloride. He says it does not increase Bile, and too large a dose diminishes it.

The question has been raised as to what share the stomach takes in the act of vomiting. Some say that it is entirely passive, the act being caused by the pressure of the diaphragm and abdominal muscles; others assert that the stomach performs the essential part of the process, the diaphragm and abdominal muscles being merely accessory. Majendie's experiments, which seem to reduce the action of the stomach to a passive state in this process, are entirely fallacious. The substitution of a bladder for the stomach; the circumstance of its only possessing one opening corresponding to the cardiac orifice, without a second to answer to the pyloric; and the unnatural and painful conditions in which the subjects of the experiments were placed, fully constitute the "experimentum fallax," and show that experiment differs widely from experience and correct observation. Majendie, in this as on other occasions, did not take into account the various sources of error to which experiments on living animals are liable. He did not consider the unnatural positions in which such experiments place the animals experimented upon, and thus derange their natural functions. Stricter and more comprehensive views of the

subject show, that whilst former physiologists have erred in attributing the act of vomiting too exclusively to a sudden contraction of the stomach, Majendie and his disciples have been equally to blame in adopting so implicitly the more tangible phenomena of inconclusive experiments.

On the other hand, observations of those natural cases of fistulous openings into the stomach seem to support the view that this viscus takes some part in the expulsion of its contents, aided undoubtedly by the contraction of the diaphragm and abdominal muscles, and, moreover, the contraction of the stomach itself is usually distinctly felt by the patient during the act of vomiting. Richerand felt all the absurdity of these experiments, and in reference to them remarked that "careful examinations of the phenomena of disease are more calculated to throw light upon physiology than painful experiments performed upon the lower animals. Many pathological facts may be adduced in corroboration of the explanation I have given of the phenomenon of vomiting, and in support of it I may refer to the conclusive observation and reasoning of Professor Lallemand and Monsieur Bourdon on this subject."

Now Richerand was a vivisector!

The experiments that have been performed upon the large intestines are useless, since many cases of fistula of the colon (the result of disease) are recorded, presenting every requisite for observation; the operation of colotomy, cases of strangulated hernia, and wounds of the intestines offer every opportunity for physiological and pathological research. We can always profit by the misfortunes of others without putting animals to pain; it is absolutely going out of our way to vivisect when we have always at hand the very pathological conditions required, without their being artificially produced.

Disease of the kidneys is a very common occurrence; the symptoms are well marked, and when the renal organs are rendered incapable, by disorganisation, of further action, or

when an obstruction exists in the ureters, the urea, instead of being eliminated, circulates in the blood producing uræmia, accompanied by convulsions and coma. By such symptoms as vomiting, pneumonia, and profuse perspirations, together with an ammoniacal smell of the body, we become aware of the fact that the stomach, lungs, and skin are endeavouring to remove the poison. All these are plain indications of the course that nature is taking to remedy the evil, and yet physiologists, dissatisfied with this teaching, have tied the ureters of animals with the result of obtaining the same symptoms as they had every day beheld in natural disease. Again, Bernard, by removing a portion of the skull and irritating the pneumogastric nerve at its origin in the floor of the fourth ventricle, induced diabetes. On the other hand, Dr. Goolden, by observations on patients, ascertained that blows on the head were a frequent cause of this disease. Here is again a case of observation *versus* vivisection, and it seems more than probable that such an operation as removing the skull and irritating the brain would have very much the same effect as the irritation produced in the brain by blows, and hence careful observation is preferable to experiments of this kind. Over-exertion of mind and body is known to be a cause of diabetes; this mental activity evidently irritating some part of the brain or lowering its vitality, so that the nervous impulse transmitted to the liver is perverted or disordered, and consequently does not regulate properly the functions of that viscus. We have so many theories of the origin of this disease that one is at a loss to know which to adopt; the only certain thing about it is that these experiments have rather tended to cloud than enlighten us. Treatment has not been altered or improved by these theories; common-sense is still left to us for our guide, and by it we are enabled to effect more than by attending to the results of the numerous experiments which rather help us off than on to the right path. We have but to glance at the two conflicting ideas of the

present day respecting the glycogenic function of the liver, in order to ascertain the actual amount of good practitioners of medicine may be expected to derive from such contradictions. Briefly, that of Bernard is, that the *healthy* liver forms the sugar; that of Dr. Pavy, that the healthy liver *never* forms sugar! So much for physiological experiment! The results of many painful operations on animals lead two eminent men to exactly *opposite* conclusions!

Physiologists, only within the last three years, have endeavoured to prove by experiment that hæmorrhage into the substance of the brain is attended with congestion of the kidneys and albuminuria. Schiff is of opinion that this is due to paralysis of the vaso-motor nerves, but Brown Séquard's idea is that these phenomena result from spasms of the arteries and veins, leading to engorgement and rupture of the capillaries. It must strike any one that these experiments were totally unnecessary, seeing that treatment would not be affected in any way whatever, whether the albuminuria depended upon vaso-motor paralysis, or upon arterial and venous spasm. The results obtained have but the questionable advantage of showing us what we previously knew, how remote irritation in one part of the body induces a serious disturbance in other parts, and thereby tending to obscure the true physiological phenomena sought for.

It has been noticed that after division of the nerves in a limb of an animal the temperature falls. Independently of an operation of this kind, this diminution of heat has been remarked in limbs deprived of nervous influence by paralysis. Mr. Earle found the temperature of the hand of a paralysed arm to be 70°, while the hand of the sound side had a temperature of 92° F. On electrifying the paralysed limb the temperature rose to 77°. Such common occurrences as paralysis of any part of the human body have been ample to demonstrate the influence of the nervous system in regulating the heat of a part. It is evident that the

divisions of nerves alluded to, were experiments which could at the most, only confirm what observers already knew; but when the facts and symptoms of paralysis have been the subjects of every-day study, it seems a useless waste of time to spend so much thought over facts with which the world was already sufficiently acquainted.

The lacteals were first discovered by Caspar Asellius, in the year 1662, by accident, as he himself informs us; but Eristratus had seen them many centuries before this. Bartholini and Rudbeck by chance hit upon the lymphatics while busy tracing the lacteals.

Asellius imagined that the whole of the lacteals united in the pancreas, and were thence continued to the liver. The numerous vivisection experiments performed by De Bach, led him to uphold the conclusions of Asellius. Bartholini, by a course of dissections upon the *dead body*, showed the absurdity of this theory, and amply demonstrated their true course. The lacteals were therefore discovered by mere chance, and the direction of their contents wrongly stated, as the result of vivisection enquiries. Since the time of the earlier vivisectors, physiologists have sacrificed innumerable animals to ascertain the true functions of the absorbent system, and an exposure of their fallacies, as well as conflicting statements drawn often from the same series of experiments, would be ludicrous were it not painful. Before the discovery of the lacteals, it was believed that absorption was carried on entirely by the arteries and veins; after their existence had been demonstrated, the theory was that the blood-vessels co-operated with the lacteals in carrying out this function. In the year 1758, John Hunter performed a series of cruel operations on dogs, by opening the abdomen and tying a piece of intestine and the mesenteric artery and vein, or by tying intestine only. From these experiments, he concluded that the lacteals and lymphatics possessed the power of absorbing any substance, and that the veins had no share whatever in this office.

Then Majendie comes upon the scene and denies the truth of Hunter's conclusions, asserting that the lymphatics have no power of absorption, which function is carried on solely by the blood-vessels; nevertheless, from more experiments performed at the Academy of Medicine of Philadelphia, it would appear that the lacteals *have* the power of absorption! Now, when so many illustrious physiologists disagree in every important particular, after having caused the mutilation and death of many animals, How are we to come to any definite opinion on this subject? Each vivisector comes to us with a different theory and asks us to believe it, and during the whole time that these theories are being worked out, not one can state with any certainty the precise structure of the villi and lacteals. Some say that both villi and lacteals are perforated at their extremities, others deny this; therefore it seems evident that until the minute anatomy of the part is thoroughly ascertained, no true knowledge of its function by any vivisection process will ever be gained.

Valentin, disgusted with experiment, turned to anatomy, and endeavoured to prove that from the identity of arrangement of the blood-vessels and lacteals in villi, with that of the vessels and ducts in some secreting glands, the chyle is absorbed first into the blood-vessels, and thence, as it were, secreted into the lacteals. Finally comes the last theory, that the chyle is absorbed into the lacteals through the instrumentality of cells. Richerand points out that physiologists have erred, in relation to the early stages of the function of absorption, in neglecting to appreciate the effects which foreign substances have upon the different textures both during life and after its extinction. Thus saline matters in a state of solution, putrid animal substances, numerous vegetable products in a state of fluidity act in a most evident manner upon the animal textures when the animal to which they belong is deprived of life, and whilst they yet retain their specific characters.

How long does it take for substances introduced into the stomach to appear in the urine? This question has formed the basis of numerous experiments, consisting in dividing the ureters at their connection with the bladder, and then fixing them to an artificial bladder. Now cases of epispadias are by no means uncommon, where there is a complete absence of the anterior wall of the bladder, the abdominal wall, and the pubis symphysis. There is a deep gap at the lower part of the abdomen from which the mucous membrane of the bladder protrudes, and the orifices of the ureters are seen, from which the urinary secretion is constantly dribbling. The necessity of operating on animals to produce a similar condition to this is somewhat more than doubtful. Here are, as usual, cases to hand of malformation of the human being, and experiments can be made upon them without the least pain, as has been done by Erichsen upon infants. Whence, then, the object of this gratuitous cruelty?

It is very evident that, notwithstanding the immense amount of labour bestowed upon this subject, we are almost, if not quite, as much in the dark as before; and when we read of one physiologist exposing the fallacies of another, and he himself being taken to task by a third, and so on, can it be wondered at that the vivisection method of experiment should be decried as useless, as well as being productive of intense cruelty and suffering!

One gentleman, referring to the incredulity expressed by Tiedeman, Gmelin, and others as to the power of the lacteals to absorb colouring matter and salts, suggests that, as the kidneys generally excrete these substances very rapidly if present only in small quantities, these organs should be removed so as to permit the salts to be absorbed into the lacteals. Hence, in addition to cutting open the abdomen, exposing and ligaturing the intestine, tying the mesenteric arteries and veins, and disturbing the functions of the parts by lowering the temperature of the bowel through its exposure to the air, we must still eliminate the influence of

the kidneys by removing them, this of course involving ligatures on the renal arteries and veins and the ureters. After doing all this we are then expected to settle the long-vexed question of absorption by the blood-vessels and lacteals! Undoubtedly, the mere fact of including the many ramifications of nerves in the parts ligatured would, in the eyes of a vivisector, not materially disturb any vital function; nervous influence, shock, and the great alteration of function induced by the hideous mutilation described, might not possibly re-act upon the absorbent system. But how can we prove it? A philosopher is endeavouring to discover such or such a function, he is aware that all parts of the system sympathise intimately with each other; how then can he guage the effect produced by some sudden disturbance of all the functions of the body upon this particular unknown function which he is endeavouring to discover? It might retard it, it might accelerate it, it might suspend it, or it might induce a vicariousness. Is it possible, under these circumstances, that a normal law can ever be established such as will be in harmony with clinical or pathological teaching? Reason disdains to answer in the affirmative, and points with scorn to the miserable conjectures hazarded one after another only to be destroyed, and hydra-headed, to rise up again in new forms until legitimate research establishes a law capable of bearing the severest test of truth. Such is the state of our knowledge upon absorption. We are still groping in the dark, and vivisection has not let in the light upon us; from beginning to end it has misled us both as to the functions and the anatomy of the absorbents. Asellius, by vivisection, traced the vessels to the pancreas and liver, but Bartholini, in the dead body, showed their proper distribution; Hunter, Majendie, Tiedeman, Valentin, *cum multis aliis*, contradict themselves and one another. Whom, then, are we to believe?

It has been asserted that our knowledge of dropsies, apoplexy, and all diseases depending upon altered blood-

pressure, as well as the use of transfusion of blood as a cure in severe hæmorrhages, is dependent upon Harvey's discovery of the circulation of the blood, acquired through vivisection. But assertions of this description, though easily made, are not quite so easily susceptible of proof, and a careful perusal of Harvey's great work shows that vivisection had absolutely nothing whatever to do with his discovery of the circulation. Moreover, it can be shown that the idea of a circulation was held previous to Harvey's time, as the following passage from the "Timæus" of Plato tends to demonstrate:—"Now after the directing artificers of our structure had implanted all these organs for giving nourishment to our inferior nature, they directed various channels through our body, so as to water it like a garden, by the constant accession of *flowing* moisture . . . and that the flood (of blood) supplied thence to other parts might give an equable irrigation."

Passing from Plato, we find that Galen, by means of vivisection, exploded the erroneous doctrine that arteries conveyed vital spirits only; but in the sixteenth century the vivisector Vesalius contradicted the result arrived at by Galen, and taught that while the arteries were merely the conductors of vital spirits from the heart throughout the body, the veins were the principal vessels, the real blood and the function of nutrition being exclusively ascribed to them.

The general belief at this period was that the blood circulated to and from the vessels, inspiration propelling blood into the vessels, whilst expiration restored it to the heart, but nevertheless the way was being gradually paved for the abolition of this erroneous doctrine by the accumulation of a mass of anatomical facts, which proved subsequently of such enormous utility to Harvey. Berengar described the valves of the ascending cava, of the pulmonary vein and the other cardiac valves—Cannani (1547), Sylvius, Stephanus (1536), Eustachius (1560), Alberti, Sarpi, and

Fabricius (1574) observed *valves* in most of the veins of the human body. Fabricius made drawings of them and explained their use, stating they were designed by nature to prevent congestions as well as too great dilatations of the veins. "In the arteries," said Fabricius, "the valves are unnecessary, as the influx and reflux of the blood here is not so much interrupted as in the veins." This passage shows that Fabricius only just missed conceiving the principal use of the valves, that of promoting the return of the blood to the heart.

Towards the end of the sixteenth century many anatomists began to adopt the theory of the smaller circulation through the lungs. But before this theory could be accepted, much depended upon a thorough examination of the septum between the ventricles of the heart, which Galen considered as perforated, so as to permit the blood to pass from one ventricle to the other. Berengar found the septum so solid that he denied the possibility of such a passage; and as this notion began to prevail, anatomists conjectured that the vena cava arose from the heart, and not, as previously imagined, from the liver. "If," argued they, "the vena cava arose from the liver and conducted the blood to the heart, then the aorta, which, besides vital spirits, also contained blood, must either receive this blood from the pulmonary artery *after* it had passed through the lungs, or there was no other way of accounting for it than by admitting the exudation of blood through the septum." Laguna attempted to overcome this difficulty by asserting the perforation of the septum, the passage of part of the blood from the right to the left ventricle, another part flowing through the pulmonary artery to the lungs for their nourishment. Arantius hesitated to believe that blood could penetrate through the solid septum from the right to the left ventricle. He argued that if such were possible, why could not the blood also pass from the left into the right ventricle, "and thus, perhaps, disturb the order of

nature." Again, "of what use would the coronary veins and great pulmonary artery be if the blood was merely to penetrate through the septum from one ventricle to the other?" "Why was the pulmonary artery so large if it was only to convey air from the lungs, and not also blood to the heart?" "Nay, *after death*, we frequently find the pulmonary artery quite full of blood." Michael Servetus maintained the complete impermeability of the septum, and applied it to explain the circulation through the lungs. In his "Restitutio Christianismi" (Lib. v., p. 169; Edit. 1790), he says:—"The vital spirit of the arteries penetrates, through their anastomoses with the veins, into the latter; for, according to the previous assertion of Vesalius, every vein in the different parts of the human body is most intimately connected with an artery. It is impossible that the blood can pass through the septum from the right into the left ventricle of the heart, because the septum is quite impermeable; hence it must pass through the lungs; here it receives fresh vital spirit from the atmospheric air, and thus it again returns from the lungs to the heart." Servetus also came to the conclusion that the pulmonary artery does not nourish the lungs, because it is uncommonly large and wide in proportion to its vein, because it is accompanied throughout by its vein, and because there are other vessels designed by nature for the support of the lungs. He further affirmed that the accession of the vital spirit cannot take place in either of the ventricles of the heart.

It will be observed that anatomical observation was the sole means whereby the smaller circulation through the lungs was established; and it would almost stand to reason that if a true circulation be found in one part of the body, the other parts would also participate in this method of the distribution of the nutritive fluid. James Rueff, in a work published at Zurich, in 1554 (a year after Servetus), treats of the distribution of vital spirit throughout the *whole* body by the arteries (*De Conceptu et Generat.*; Lib. i., c. 4), and

Garengeot claims for Rueff the discovery of the *great* circulation of the blood.

It seems, therefore, that upon this evidence the claims of Harvey as a discoverer are somewhat smaller than is generally imagined. Nearly a hundred years before his time we find the circulation through the lungs accurately described, and the greater circulation distinctly shadowed forth. All the anatomical facts necessary for the circulatory theory had been gathered together, and it only now remained for one master-mind to arrange these facts into one consistent theory which would stand the severest tests that could be brought against it. Such a master-mind was William Harvey, who extended the theory of the circulation through the lungs so as to embrace the whole system.

Yet once again, before the time of Harvey, Cæsalpinus (about 1570) in his "Speculum Artis Medicæ Hypocritum," shows very clearly that he had a knowledge of the general circulation. "In animals," he writes, "we see that the nutriment is carried through the veins to the heart, as to a laboratory, and its last perfection being there attained, it is driven by the spirit which is begotten in the heart through the arteries, and distributed to the whole body."—(Book I., Chapter II.) It is impossible not to remark on this most suggestive passage, that the theory propounded since the time of Harvey could scarcely be more accurately or definitely stated, and it is evident that Cæsalpinus could not have acquired this knowledge from the works either of Fabricius or Harvey. Besides this, Bartholini, Professor of Anatomy at Copenhagen from 1648, declared that Pauli, of Venice, was acquainted with the circulation previous to Harvey. Fabricius ab Acquapendente, the tutor of William Harvey, taught the latter the valvular structure of the veins. The arrangement of these valves so as to permit the flow of blood only in one direction, and preventing regurgitation, must have struck anyone almost at his first examination of them. With this mass of evidence collected for him,

Harvey's shrewd mind must easily have *re-discovered* the truth; and he, more fortunate than his forerunners, succeeded, after a storm of opposition, in convincing the world of the stability of his theory. It is no doubt true that Harvey attempted to discover the circulation by vivisection, but so perplexed did he become that he threw this method of research overboard. He thus describes the bewilderment vivisection led him into:—"When I first gave my mind to vivisection as a means to discovering the motions and uses of the heart, and sought to discover them by actual inspection, and not from the writings of others, I found the task so truly arduous, so full of difficulties, that I was almost tempted to think with Fracastorius, that the motion of the heart was only to be comprehended by God; for I could neither rightly perceive at first when the systole and when the diastole took place; nor when and where dilatation and contraction occurred, by reason of the rapidity of the motion, which in many animals is accomplished in the twinkling of an eye, coming and going like a flash of lightning, so that the systole presented itself now from this point, now from that. The diastole the same. Then everything was reversed; the motions occurring, as it seemed, variously and confusedly together. My mind was therefore greatly unsettled; nor did I know what I should myself conclude, *nor what believe from others.* I was not surprised that Andreas Laurentius should have said that the motion of the heart was as perplexing as the flux and reflux of the Euripus had appeared to Aristotle."

Perplexed and baffled, Harvey then went back to examining the anatomy of the dead body, and came to the conclusion that the blood must flow back again from the arteries into the veins by certain secret passages, or by pores of the flesh, or by mutual anastomoses of the arteries and the veins; he demonstrated that the last-mentioned communication takes place in the lungs. He thus discovered by dissection of the *dead body* that the arteries and veins were

continuous in the lungs, and if this happened in one part the inference would be that it happened everywhere throughout the body. Having ascertained this continuity of the two different sets of blood-vessels, he then proceeded to reflect upon the uses of the valves of the veins:—"Moreover, it must be evident to everyone," he observed, "who considers the situation and connection of the valves, that the blood passes from the smaller branches of the veins into their trunks, and from thence to the heart."

Harvey might have saved himself a vast amount of labour had the simple expedient of injecting the vessels through the arteries been adopted in the dead body. A fluid, different in colour from that of the blood, would be perceived after injection at certain intervals of time to pass through every artery and vein, and finally return to the heart. So simple was the whole theory to discover after all the mass of facts had been collected, that we may exclaim, with Dr. Lauder Brunton, that "Harvey himself was led to form his ideas regarding the course taken by the blood, from the position of the valves in the veins, and might possibly have been able to describe it exactly, *without making a single experiment.*"

Such was the discovery of the circulation of the blood by careful observation and dissection, after vivisection had proved itself to be worse than useless by confusing the mind instead of enlightening it; a proof, as must ever be insisted upon, of the barrier that nature places against any kind of research based upon a violation of the moral law.

Depending upon Harvey's discovery, transfusion is pointed to as one of its outcomes. But since the circulation theory was not the result of vivisection experiments, it is evident that the cutting up of live animals had nothing to do with the discovery of transfusion. Even if it were established by vivisection, it is an operation that surgeons have no particular reason to be proud of, considering that it is so seldom successful that even as a *dernier resort*, one is loathe to put

it into practice. However, unfortunately for the vivisectors, it is nearly certain that this operation was known and practised whilst Harvey was in his earliest infancy. The first idea of transfusion is due to Libavius, who recommends it to be performed from artery to artery, certainly a difficult and dangerous task. The following extract from the "Philosophical Transactions," for the year 1668, shows that Harvey cannot claim the operation for himself:—"There has been of late some contest about the origin of transfusion, the English first claiming it as a late invention of their's, the French pretending thereupon that it had been proposed among them ten years ago; after which, it was affirmed upon further investigation, by some ingenious persons in England, that it had been known there thirty years ago. But it seems that an Italian philosopher in a tract entitled, "Relatione dell' Esperienze fatte in Inghilterra, Francia e Italia, intorno la Transfusione del Sangue," lately printed in Rome, undertakes to prove that the transfusion is yet of greater antiquity, as having been known to Libavius above fifty years since. For which that Roman author quotes a place out of the said Libavius (In Defensione Syntagmatis Arcanorum Chymicorum contra Heningum Schneumannum, Actione 2, p. 8; Edit. Francof. A. 1615), where the transfusion is so plainly described that one can hardly discourse of it with more clearness than is there done in these words:—"Adsit Juvenis robustus, sanus, sanguine spirituoso plenus; Adstet exhaustus viribus, tenuis, macilentus, vix animam trahens. Magister Artis habeat tubulos argenteos inter se congruentes, aperiat arteriam robusti, et tubulum inserat muniatque; mox et ægroti arteriam findat, et tubulum fœmineum infigat. Jam duos tubulos sibi mutuo applicet, et ex sano sanguis arterialis, calens et spirituosus saliet in ægrotum, unaque vitæ fontem afferet omnemque languorem pellet."

As recently as this year, vivisectors have upheld the operation of transfusion as being now "often performed in

urgent cases with the result of saving many lives."—(*Brit. Med. Journal*, April 10, 1880.) The week following this laudation of the happy result of vivisection appeared an article from the pen of Dr. Madden, from which it will be seen that (like the late Claude Bernard's almost dying words) the operation is nearly futile now, but may be more promising in the future! He says:—"The success of transfusion in a few cases of collapse from flooding is hardly sufficient to warrant much dependence on it, save as a last resource. At some future time this operation, in an improved form, is *likely* to fill a prominent place in medical and surgical as well as in midwifery practice; but at present, its uncertainty, risks, and difficulties are only equalled by the gravity of the condition in which its performance is justified. The promises held out when upwards of half a century ago Dr. Blundell revived transfusion, are apparently as far as ever from being realised. Nor have any of the more recent suggestions . . . . . afforded much better practical results than Dr. Blundell accomplished."

It is difficult to state with any certainty whether our modern treatment of apoplexy or pulmonary congestions has very much the advantage over that adopted previous to the discovery of the circulation. It would be interesting to know positively whether Harvey's discovery has really lessened the mortality from these diseases. But it is known that that part of these diseases dependent upon altered blood-pressure is still treated as it was before a circulation theory was thought of; for we find that Celsus recommended bleeding, and this treatment is, even at the present day, frequently our sheet anchor. Again, with respect to dropsy, in this enlightened nineteenth century, how often do we hear of its cure? The only means we can make use of is that of palliation by tapping; but it will be found that this operation was practised in the time of Celsus, who describes the method of performing it (vii., 15). Moreover, the discovery of the circulation did not convince everyone of the

true pathology of this disease, for we find that as recently as the end of the last century, dropsy was considered by many eminent pathologists to depend upon rupture of the lymphatics. Amongst those who upheld this theory may be mentioned Assalini, Sœmmering, Haasse, and Von Swieten.

Since the time of Harvey, innumerable experiments have been performed with the object of discovering the true physiology of the circulation, and in these experiments contradictions and fallacies are as apparent as elsewhere. The results of some of these operations led many physiologists to the conclusion that it was easy to see that the auricles and ventricles dilate and contract alternately, the motion being so managed that contraction of the auricles takes place simultaneously with the distension of the ventricles, and *vice versa*. Dr. Hope animadverts very strongly upon the means whereby this alternate theory was built up, and remarks that it is easy to see how such errors are rendered possible, by the fact that the experiments were performed on *living animals*. He goes on to say:—"I have always found that when the animal unfortunately retained or regained the slightest degree of sensibility, the action of the heart was so violent, convulsive, and rapid as to present the alternate action described by Majendie." In asses, poisoned with curari, much the same appearance is presented.

"Let us enquire how the heart would act on the alternate principle, by an appeal to physiological principles, in large animals with a pulse of 50, as in many human subjects, or of 40 or less, as in horses; or let us take for illustration a still more striking case of a gentleman with a pulse of 28, without the slightest intermediate beat or sound of the heart. The second sound follows the first almost as quickly as when the pulse beats 60 or 70; consequently there is a period of repose of about a second and a quarter, as three-fourths of a second suffice for an ordinary systole and diastole. What then, on the 'alternate theory,' must be

the state of the heart during this second and a quarter of repose? The ventricles *must* be in a state of diastole, because this state follows the second sound. Now, the ventricular diastole, says Majendie, is synchronous with auricular systole; consequently, the auricles, after their systole must remain in the state of spasmodic constriction for the period of a second and a quarter, waiting for the next contraction of the ventricles, which are unexcited by the stimulus of distension. This is monstrous! Its physiological impossibility is palpable. How much more simple and natural and beautiful to suppose that the relaxing ventricles refill, without distending themselves, from the auricles, and that the whole organ thus remains in repose till the progressive venous influx provokes the next auricular contraction! How admirable the arrangement by which the auricles tranquilly deliver their blood at twice, instead of the single violent alternate delivery supposed by the view of Majendie. Fortunately, his high authority is opposed by that of Harvey and Haller." Bouillaud appears to follow Majendie, and this error has led him into several others respecting physical signs. Dr. Bostock, whilst following Majendie, commits the extraordinary indiscretion of applauding Haller's experiments which coincide with Hope's, and are opposed to the "alternate theory" of Majendie!

Dr. Hope acknowledges his experiments on the heart to have had no other object but that of illustration, for he remarked, "From experiments on small animals, *supported by analogical arguments derived from pathology*, I had previously been able to *infer* the nature of the heart's action!"

In his experiments on asses, to discover the source of the cardiac sounds, he tells us that on the exposure of the heart its action became quick, tumultuous, quivering, and irregular, then slower, and finally so feeble that the second sound was never very audible. Although Dr. Hope asserts that in

succeeding experiments the heart's action became more regular, yet the fact of the violent disruption of the chest walls, the admission of air into a previously air-tight cavity, and the known tumultuous cardiac action as the result of this mutilation, are sufficient to show that, even under the most favourable circumstances, the healthy action of the heart must be suspended, and an erratic morbid one substituted.

Again, Hope refuted Majendie's theory of the cause of the first and second heart-sounds by an appeal to that pathological state known as "hydropericardium;" he also takes Bouillaud to task for teaching that the obstruction to the circulation in valvular disease is due solely to valvular contraction, and that hypertrophy has no share in it, "for," says Dr. Hope, "such a doctrine is not only erroneous, but dangerous, as it leads to pernicious practice."

Truly, all these discordant interpretations of experimental physiology are very bewildering, but they tend to exemplify the endless variations of opinion which a pursuit of this kind, loaded with error, must give rise to.

With reference to Hope's experiments, his own words tell us that Harvey and Haller rendered any others unnecessary, whilst all the most important conclusions are drawn from pathology and not from experiment.

Very recently, the public has been somewhat authoritatively informed that our knowledge of the heart and its functions has been placed upon a new and improved basis by certain experiments performed by Dr. Billing and others about fifty years ago. "They made experiments on animals by hooking back the valves of the heart, and so ascertained the cause of the natural sounds, and how bruits are produced." Although these experiments were made fifty years since, there still, at this present time, exists great discrepancy of opinion as to the cause of the natural sounds. According to some, the first sound is due almost entirely to the noise or bruit caused by the forcible contraction of the

muscular fibres of the ventricles; others refer it exclusively to the vibrations of the auriculo-ventricular valves suddenly rendered tense by the backward pressure of the blood against them, whilst a third opinion is that the sound results from the combined effect of both these agencies, aided also by several other attendant circumstances. The theory of the cause of the first sound being valvular has been supported by Dr. Halford, who endeavoured to show experimentally that the tension and vibration of the valves resulted principally from the forcible impulse of the blood against them; "but," says Mr. Kirkes, "although this experiment proves the large share which the valves take in producing the first sound, yet it does not demonstrate that it is due wholly to this cause." The plan adopted in this experiment was simultaneous compression of the pulmonary veins and vena cavæ, whereby the entrance of blood into the heart is prevented, resulting in an almost complete cessation of sound. This method obviously cannot lead to any accurate conclusion, for the exclusion of blood from the heart not only interferes with the vibration of the valves, but arrests any sound that might arise from the vibration of the blood itself in its rapid propulsion from the ventricles; and considering that the empty heart has nothing to contract upon, it would annihilate the sound caused by the impulse of the distended ventricles against the chest wall, and it precludes also the possibility of sound being generated in the walls of the ventricles and the arterial coats, all of which, as well as the auriculo-ventricular valves, are tense and susceptible of vibration at the time of the ventricular contraction.

From the uncertain results of this and similar experiments, we may still consider the cause of the first sound to be undiscovered, but there appears to be less doubt about the second, which is generally attributed to the sudden closure of the semi-lunar valves when the ventricular systole ends. Disease of these valves, which interferes with their

efficient action, demonstrates this fact by modifying or destroying the distinctness of the second sound.

Notwithstanding assertions to the contrary, it seems very evident that our knowledge on this subject is not so accurate as some would have us to believe. Halford's experiments only demonstrated a portion of the truth, whilst the method of their performance most obviously precluded any chance of ascertaining exactly the actual cause or causes of the sound; morbid anatomy has taught us as much on this point as vivisection has—the latter simply illustrated what valvular disease is daily teaching us.

The functions of the heart are chiefly due to the ganglionic system of nerves bestowed upon its structure, and experiments have been performed to prove the accuracy of what must reasonably have been considered to be true.

Willis divided the eighth pair of nerves in the neck with the view of paralysing the action of the heart, but death did not supervene for some hours, in some cases not until several days after the operation. The brain and spinal cord have been destroyed, but beyond modifying the heart's action, the operation failed in its object. In experiments performed on fishes, cardiac action has continued long after the destruction of the cerebro-spinal mass; and many of the lower animals have continued to live for a considerable time after decapitation. These experiments prove that the rythmic nutrition of the heart is due to the sympathetic ganglia, and not to the pneumogastric or other nerves; but, nevertheless, they were useless and therefore cruel, with the exception of the decapitation ones, which last were amply sufficient to eliminate the influence of the brain and cerebral nerves from the question. But nature has supplied us with cases which show incontestably the sole government of the heart by the sympathetic system. Fœtuses have frequently been born without brain, or without either brain or spinal cord, and yet the action of the heart and circulation has been perfect. Mr. Lawrence, in the transactions of the

Medico-Chirurgical Society (vol. v., p. 165), relates a case of a child born without a brain, which lived four days. These cases, by removing any chance of the existence of a pneumogastric nerve, ought to have been sufficient to have prevented experiments upon living animals, loaded, as they must necessarily be, with every element of fallacy.

These anencephalous fœtuses soon die, so that nature presents us with the advantage of watching symptoms during life, and the power of justifying our inferences after death. There need, therefore, be no disturbing influence on the circulation by performing a painful experiment; everything becomes easy by observing nature, whilst everything is difficult if we act in defiance of her laws.

The experiments which have been devised and practised for ascertaining the functions of the nerves are amongst the most painful that can be imagined, and the results thereby obtained have generally been both unconvincing and uncertain.

As an example of this form of experiment, the following one of Brown-Séquard may be cited:—"If, in a frog, the three posterior roots of the nerves (spinal) going to the hinder extremity, be divided on the left side, and the three anterior roots of the corresponding nerves on the right side, the left extremity will be deprived of sensation, the right of motion. If the foot of the right leg, which is still endowed with sensation, but not with motion, be cut off, the frog will give evidence of feeling pain by movements of all parts of the body, except the right leg itself, in which he feels the pain. If, on the contrary, the foot of the left leg, which has the power of motion but is deprived of sensation, is cut off, the frog does not feel it, and no movement follows, except the twitching of the muscles, irritated by cutting them or their tendons." The object of this experiment is to show that the anterior roots of the spinal nerves are motor, whilst the posterior roots are sensitive. Now, Sir Charles Bell had already conclusively proved this, and, therefore,

these experiments were useless. But it has been long ago shown that the application of electricity in a body recently dead is quite sufficient to prove these facts. Electricity is an agent capable of distinguishing the motor and mixed nerves, and MM. Longet and Matteuci, in alluding to the different and remarkable action of electric currents on nerves, observe that this method appears "to constitute a sure means of distinguishing these nerves from each other, and consequently serve to elucidate a question which has hitherto divided physiologists."

Numerous experiments upon the sympathetic system of nerves have, as might be expected, failed in their object, as the experimenters have been unable to eliminate the influence of the cerebro-spinal system. Thus, in experiments "now more than sufficiently numerous" (as Mr. Kirkes said), irritation of the semi-lunar ganglia, the splanchnic nerves, the thoracic, hepatic, and other ganglia and nerves, *have elicited expressions of pain!* To account for this, we are told that we must *assume* that the conduction of impressions is effected through the cerebro-spinal nerves' fibres; but vivisection gives no absolute proof that this is a fact, and regards it simply as an assumption, as "there are no means of deciding this." We must also *assume*, that if the conduction is effected through the cerebro-spinal nerves, the ganglia modify the impulse! If we did not suppose all this, we should be landed in this absurdity—that the motions of involuntary organs and muscles are under the control of the will, a theory which is palpably wrong. All these experiments have simply led us into the region of supposition! But physiology requires something more than this; we are led to believe that vivisection can lay down true landmarks, but every experiment hitherto examined, in any department of physiology, has been found to be conceived and carried out in error, or else has merely corroborated facts previously ascertained by clinical or pathological research. The great error in this series of experiments has been the attempt to

identify, by unnatural methods, functions which are most intimately and indissolubly mixed up, through their nerves, with the whole of the cerebro-spinal system.

The discovery of the functions of the anterior and posterior roots of the spinal nerves was effected by Sir Charles Bell, in spite of the opposition his views had to undergo from the somewhat contrary results obtained by vivisection. He was led to this discovery by a chain of reasoning of a highly philosophical character. His attention was first attracted by the difference in the distribution of the nerves of the head from those of the body, and by the fact that all the spinal nerves arise by double roots. Observing that this form of origin was the same in all animals possessing a spinal cord, and considering that the anterior column of the spinal marrow was continuous with the crura of the cerebrum, and the posterior with the crura of the cerebellum, he came to the conclusion that the anterior column of the spinal cord was different in function from the posterior, and that through the former the simple voluntary power of moving particular parts was conveyed. He deduced this from observing that the two nerves, which were generally supposed to be purely motor, arise from the anterior fasciculus.

These important discoveries were therefore obtained by pathological observation combined with an accurate knowledge of anatomy, both human and comparative, and a capacity for acute reasoning. His views, however, were disputed by other physiologists, who maintained that the anterior and posterior roots of the nerves were both concerned in the reception of impressions and the production of motion. This latter theory was based upon vivisection experiments, whereas Sir Charles Bell's opinions were the results of careful observation; for his experiments on animals were only performed subsequently to his discovery, and were made for the sole purpose of convincing those who were sceptical, but it does not appear that they were

very successful in proving his case. Which theory has stood the test of all these years? Which, according to our present knowledge, is the most correct, that obtained by vivisection or that resulting from anatomical research? It is well known that the steps which led up to Sir Charles Bell's discovery constitute a chain of most masterly reasoning, which will stand the test of all time. There did exist, at one period, a chance that the obscure results of vivisection experiments would, by their adverse decision, retard the truth on this question, but fortunately it was otherwise.

It would be impossible to quit this subject without paying tribute to the unparalleled genius of Bell, who may be ranked, as a discoverer, as the equal of Harvey. Before the time of Bell, all nerves were held to be alike in character, and were considered simply to give more or less nervous susceptibility to any organ, in proportion to the numbers in which they were distributed. Bell discovered and showed that the nerves were *naturally* distinguished among themselves, and *clearly classified;* and that the nerves of sense (whether peculiar or general), and those of motion, were totally distinct in their character and origin. He, in fact, laid bare, for the first time, the great fact of a distinction existing in the nature and quality of the nervous energy, which previously had all been huddled together under one interpretation. He was enabled to lay down clear truths from his profound knowledge as an anatomist, and if anyone should attempt to claim his discoveries as the outcome of vivisection, his own words prove the reverse, and, moreover, convey a stinging rebuke on the practice:—"The opening of living animals has done more to perpetuate error than to enforce the just views taken from anatomy and the natural sciences." Nothing is plainer than his condemnation of the practice of vivisection, and no one regretted more than he did the few painful experiments he perpetrated. Numerous physiologists have attempted to solve the problem as to the functions of the cerebellum. Flourens, Bouillaud, and

others have extirpated it by successive layers, and have thus shown that the power of combining the action of the muscles is thereby lost. Even if this be true, numerous cases of disease of the cerebellum have taught us just as much, and comparative anatomy has thrown great light upon the subject by showing that in each of the four great classes of vertebrata, the species whose natural movements require most frequent and exact combinations of muscular actions are those whose cerebella are most developed in proportion to the spinal cord.

If one of the crura of the cerebellum be divided, the animal commences rotating round the long axis of its body; the rotations take place with great rapidity, and may last for several days! One must feel pity for the poor animals who were suffered to remain in this torture for several days, particularly when we know that disease of the cerebellum had been observed to produce identical effects. Similar movements have been observed in men, and Dr. Paget has collected and recorded such cases; in one man there was an apoplectic effusion in the right crus cerebelli; and in a woman an exostosis pressed on the left crus. Now these cases are not intensely rare, therefore the ingenuity of the devisers of these experiments was not required to prove what disease had already proved.

Dr. Brown-Séquard denies the inference drawn from these experiments, that the function of the cerebellum is that of co-ordination of muscular movements. He says:—"I have ascertained that it is by the irritation they produce on the various parts of the base of the brain that the diseases of the cerebellum or its extirpation in animals cause the disorder of movements which has been considered as depending upon the absence of a guiding power. In fact, the least irritation of several parts of the brain, with only the point of a needle, may generate very nearly the same disorder of movements that follows the extirpation of the cerebellum. I have thus been led to conclude that, after this

extirpation, or after the destruction by disease of a large or small part of this nervous centre, it is not its absence, but some irritative influence upon the parts of the encephalon that remain unaltered which causes the irregularity of movement."

Independently of these divergent opinions expressed by physiologists as the result of different interpretations of their experiments, we are led, by these ideas of Brown-Séquard's, to the conclusion that the cerebellum has no function at all! This is the logical sequence of his arguments, which have the demerit of proving too much. Illogical methods of research must beget illogical inferences, and consequently, we have here an example of wrong interpretation of vivisection experiments, the teaching of disease being nearly disregarded, and inferences which, carried to their logical conclusions, would annihilate all function in the cerebellum.

Division of the optic thalami and corpora striata produces strange movements, but permits of *nothing but negative conclusions*. Various explanations have been offered of those movements which ensue when the parts are mutilated; "but," as Mr. Kirkes remarks, "no such explanations guide to the true physiology of these parts."

Dr. Reid remarked, "that the experimental history of the par vagum furnishes an excellent illustration of the numerous difficulties with which the physiologist has to contend, from the *impossibility* of insulating any individual organ from its mutual actions and re-actions when he wishes to examine the order and dependence of its phenomena." The truth of Dr. Reid's assertions is amply shown by examining any of the experiments performed by division of the vagi. For the purpose of elucidation, it may be remarked that division of one vagus has no immediate effect upon the lungs, but if both vagi be divided, the animal dies very rapidly from some disorder of the digestive functions. In Reid's experiments, fifteen out of seventeen animals had their lungs

rendered unfit for the healthy performance of their functions from congestion of the blood-vessels, accompanied by effusion of frothy serum. Thus far the experiment, but then physiologists have to interpret its results, which, as usual, is done in more ways than one. Thus, one set of vivisectors argue that the first departure from health is the effusion of serum, resulting from the disorder of function induced by the section of the nerve; another set argue that the effusion is the effect of a previously congested state of the blood-vessels. It would be amusing, were it not lamentable, to see physiologists at every point quarrelling among themselves as to the true interpretation of experiments vitiated by the induced functional disturbance, and beyond the power of reason to fathom or unravel.

In this experiment, as in others, it is the difficulty of overcoming or estimating aright the amount of systemic disturbance that causes this divergence of opinion; and whilst such sources of fallacy exist, it is impossible to attach any value or significance whatever to results such as these; they simply leave us on the horns of a dilemma, and consequently the danger of applying any of these results in practice is obvious.

In the recently-dead body, on the contrary, we have the means of evoking nerve force whereby we shall obtain far more reliable information than by experiments performed during the persistence of vitality. For example, the whole of the phenomena of spasmodic asthma, and the influence of the nervous system therein, can be demonstrated in the dead body. Under the stimulus of galvanism, after the lungs are removed from the body, the muscles can be made to contract so as to lift up water placed in the trachea, in a tube. Volkman tied a glass tube, drawn fine at one end, into the trachea of a *beheaded* animal, and when the small end was turned to the flame of a candle, he galvanized the pneumogastric nerve. Each time he did so the flame was blown, and once it was blown out. A legiti-

mate experiment of this description proves that morbid excitement of the pneumogastric nerve tends to produce contraction of the muscular fibres of the air tubes. The truth of the inference drawn from these experiments is made manifest by that fact that the administration of those drugs which cause relaxation of muscular fibre invariably relieve the paroxysms. It is perfectly evident that in studying the physiology of the nerves no vivisection is necessary; galvanizing the dead body, and careful observation of the pathological condition of the living, are the safest and surest guides to knowledge.

Much attention has, of late years, been bestowed upon the morbid anatomy of patients who have exhibited definite cerebral symptoms during life, and in this way evidence of the localization of function in different portions of the cerebral hemispheres has been accumulated. Foremost amongst these newly-discovered facts is, that the left side of the body is governed by the right side of the brain, and *vice versa*, this discovery being made by observing that paralysis of either side of the body is accompanied by disease of the opposite side of the brain. This point being ascertained, little by little, other facts came flowing in, and then the idea of localization of function took its origin; though, had the doctrines of Gall and Spurzheim been more carefully investigated, we should not have to consider the idea as perfectly new. Though, undoubtedly, the system of phrenology built upon the theory was false, yet that does not necessarily prove that the theory itself was false. Had physiologists, instead of annihilating the doctrines of phrenology, carefully sifted the fundamental theories, and sought for cases of disease either to uphold or destroy them, the location of the various functions in special parts of the brain would now be a *fait 'accompli*. But alas! vivisectors are aware that the results of experiments on animals are so frequently at variance with clinical teaching that they have refused to be convinced, being persuaded that their method

is better than the plain teaching of nature. They refuse to acknowledge the lessons physicians derive from nature, and Schiff, speaking at the International Medical Congress held at Geneva, in reference to cerebral localization animadverted somewhat satirically upon the difference of opinion between physiologists and physicians, saying that the former are opposed to the doctrine, and that the latter are its only supporters. In this way experimental physiology has retarded over and over again the progress of medicine, and now at the eleventh hour, eighty years after Gall first promulgated his doctrines, we are beginning to find out that his theory was to a large extent a correct one. The idea of localization of function being once more revived, physiologists proceeded to ascertain its truth in a far different manner to Gall. The latter collected his vast store of facts from simple observation of nature, but the physiologists proceeded to interrogate nature by experimenting upon animals, utterly ignoring those plain teachings that disease is always offering to them. What easier road could we take than the study of the disease "aphasia" towards arriving at the true goal of brain knowledge? Aphasia is a disease only recently recognised, and it is characterised by a loss of the power of expressing thoughts in words. Dr. Robertson showed that aphasia patients, under violent mental emotion, will utter words and short sentences which they could not possibly articulate under ordinary mental composure, the mental emotion acting just the same as a vivisector's stimulus would to the brain of an animal under operation. In this disease there is a lesion of the efferent fibres between the external frontal convolution and the corpus striatum, and a knowledge of this enables us to localise the seat of government of articulation. Morbid states of the human body everywhere afford us the requisites for unravelling the arcana of nature; and if the phenomena of disease were more accurately attended to and interpreted, physiology would not be in the lame and halt

condition in which it is at present. Some of the conclusions at which Flourens arrived, contrasted with the opinions of the present day, are not a little remarkable. He stated that large tracts of brain cortex may be destroyed without causing any very evident mental disturbance; that any one part of the brain may be destroyed with a like result; and hence that there is no localization of function in the brain, but that each part of the encephalon is a micrencephalon, capable of itself of performing all the functions pertaining to the whole. But these conclusions are objected to on the ground that though they may be true as to the mind, they cannot be extended to physiological function. Disease here steps in and shows to her clumsy imitators that extensive lesions may exist on one side of the brain without any mental symptoms during life, but that if both sides be attacked the mind becomes disorganised. In contrasting natural disease with disease artificially produced, the above shows very plainly the extreme liability to error attending vivisection experiments; and Ferrier himself is bound to acknowledge how many fallacies physiologists are led into. He remarks that "if it is difficult to test the mental condition in a human being, how much more so must it be in the case of the lower animals." He goes on to say:—"One great fallacy has been the assumption that the results of experiments on frogs, pigeons, and other animals are capable of application to man without qualification; an assumption which vitiates the conclusions of numerous physiologists of the present day. The very fact that there exist such patent differences between the effects of the destruction of the cerebral hemispheres in different orders of animals, ought to inspire caution in the application to man of results obtained only by experiments on the brains of animals low down in the scale." Physiologists state that frogs deprived of their cerebral hemispheres are still capable of a number of complicated actions which have almost the appearance of spontaneity! Can the same be said of man?

Again, the same ablation of the brains of pigeons, rabbits, and dogs presents very different clinical symptoms to what happens in a man deprived of these by injury or disease. This difference of result has led physiologists to conclude that the cortex of the brain has no real relation to motility. But then, another set of physiologists, objecting to frog and pigeon experiments, tell us that we must operate on the monkey as being almost akin to the human species! And that what is true of the monkey is true of man! Now this method of procedure seems very like begging the question. The conclusion arrived at that "What is true of the monkey is true of man and of no other animal," seems really to imply that previous knowledge of human functions enables physiologists to say that the monkey can be affected in the same manner as man, that is an assumption of what has yet to be proved. The objections which physiologists themselves urge against frog, pigeon, and dog experiments are equally valid if applied to the monkey. Truly, vivisection logic is that of fallacy; and no one can wonder that its upholders see the necessity of qualifying their results. If physiologists were to attempt to qualify the results of their experiments, how could they be sure of applying the requisite amount of qualification, so as to render them *exactly* applicable to man? There must always be a certain amount of doubt attached to the conclusions of even the most cautious experimenter; but to quote Ferrier once more, "the slightest doubt is absolute failure," and hence we may conclude that as every vivisection operation must have some slight doubt thrown upon its result, therefore the whole series of them are absolute failures. The truth of this conclusion is shown every day by old theories being upset by new ones, and they in their turn giving way to others, and so on, until a profound reasoner shows them all to be fallacious by applying the results of patient clinical and pathological research. To sum up in Ferrier's own words once again, "Nor do the facts of experimental physiology seem so consistent with

themselves, or with *the undoubted facts of clinical research*, as to inspire us with unhesitating confidence as to their accuracy or as to their applicability to human pathology."

Whilst Ferrier thus censures the experiments of other physiologists, he himself is attacked by Brown-Séquard, who denies the truth of the law of cross action of the cerebral hemispheres, because he has observed many cases of paralysis with disease on the same side of the brain. The easiest way of getting over discrepancies between two such distinguished physiologists, is for one to assume that there were numerous sources of fallacies in the experiments of the other; and this way is not only the easiest but the most truthful.

Bouillaud, from experiments performed on animals, gave in his adhesion to the doctrine of localization of function, but he confessed at the same time that the results of his experiments were anything but satisfactory. From *clinical research*, he showed that lesion of the anterior lobes of the brain caused loss of speech, and he came to the conclusion that there must exist in the cerebrum several motor centres. This is proved by the occurrence of limited paralysis corresponding to a local alteration in the brain; for it is evident that if this organ did not contain motor centres, it would be impossible to conceive how a limited lesion could produce a limited paralysis, leaving all other movements intact. Bouillaud adds, that he is well aware that his "propositions are at variance with the results of experiments on animals," but we know that his *clinical researches* upon which he based these propositions have afforded a sure guide to the knowledge of cerebral functions.

Vaccination is pointed to by vivisectors as a most convincing proof of the good done by experimenting upon animals; but it is peculiar that they should choose this to descant upon, and thoroughly ignore the fact of its discovery being *par excellence* one against which not the

slightest suspicion of cruelty or pain can be brought. Jenner's own account of his discovery shows that no experiments were tried, but that observation established the truth of the vaccination theory. Whilst Jenner was an apprentice at Sodbury, a woman applying for advice at the Surgery informed him that she was proof against small-pox, having had cow-pox. This conversation made an impression upon him, and upon inquiry, he ascertained that the idea was traditional throughout the dairy districts of Gloucestershire and other counties. It appears that he mentioned this subject to the famous John Hunter, who, however, did not seem to be much impressed with it. Notwithstanding, Jenner continued his investigations, and found that the appearance of cow-pox at a farm was almost invariably preceded by a disease of horses' heels, known as the "grease," at the same farm. He observed that he was unable to inoculate with small-pox those who had had cow-pox. The whole of the reasoning which led Jenner to the discovery of vaccination, was based upon acute observation; and he was so satisfied with the correctness of his views, that he announced them to several medical men—amongst them, to Sir Everard Home, in 1788. In 1795, Dr. Adams mentioned the subject in his work on Morbid Poisons, but it was not until the following year that Jenner performed his first vaccine experiment, that is to say, some time after his views had been published. He gives an account of it in a letter to Edward Gardner:—"As I promised to let you know how I proceeded in my inquiry into the nature of that singular disease, cow-pox, and being fully satisfied how much you feel interested in its success, you will be gratified in hearing that I have at length accomplished what I have been so long waiting for, the passing of the vaccine virus from one human being to another, by the ordinary mode of inoculation. A boy named Phipps, was inoculated in the arm from a pustule on the hand of a young woman (Sarah Nelmes), who was infected by her master's cows. Having never seen

the disease but in its casual way before, that is, when communicated from the cow to the hand of the milker, I was astonished at the close resemblance of the pustules, in some of their stages, to the variolous pustules. But now, listen to the most delightful part of my story. The boy has since been inoculated for the small-pox, which, as I ventured to predict, produced no effect. I shall now pursue my experiments with redoubled ardour."

Such are the plain facts relating to the discovery of vaccination, and they clearly prove that Jenner's observations had been carried on for a period of nearly five-and-twenty years before he ever tried the efficacy of his theory by putting it into practice. The utility and importance of vaccination can never be denied; for whereas the old tables of mortality from small-pox showed a terrible fatality, and every second or third person being marked with the disease, we now have but a very small mortality, and the pitting is becoming a rarity. Aware of the tremendous triumph the discovery has achieved over disease, Vivisectors have not scrupled to assert that it was due to experiments on animals but the truth of this assertion is denied most palpably by Jenner's own testimony.

Dr. Baron remarks, that "the fact that the disorder in the cow originated from the horse, had not been proved by direct experiment, when he (Jenner) published his Enquiry; yet the evidence on which this doctrine rested, was so complete as to entitle it to much attention." This disproves entirely the attempt to make vaccination an outcome of vivisection, although the experiment in question would have been mild and possibly painless.

The discovery of vaccination adds one more to the long list of benefits man has derived from legitimate methods of research. Vivisection has no such triumph to record; it has never yet been the means of improving our knowledge of disease, or of modifying our treatment so as to render it more successful. The most ardent supporter of experimental

physiology can point to no discovery, the result of cruelty, which has ever alleviated human suffering, whilst we, on the other hand, can fearlessly point to all our improvements in medicine and surgery as the consequences of humane and enlightened research, unstained by cruelty.

One of the most valuable inventions of modern times has been the stethoscope. Although previous to its invention, the various sounds of the chest were understood to be of some value as a means of diagnosis in disease, yet the want of precision in recognising these sounds by simply applying the ear direct to the chest, greatly invalidated the physician's accurate knowledge of the disease. Laennec was led to his discovery (for so it must be called), by applying a well-known fact in acoustics. He states, "I was consulted by a young woman affected with the general symptoms of diseased heart, and in whose case percussion and the application of the hand were of little avail, owing to her being extremely lusty. The immediate application of the ear being inadmissible for obvious reasons, I happened to recollect a simple fact in acoustics, and fancied it might be turned to some use on the present occasion. The fact I allude to is the great distinctness with which we hear the scratch of a pin at one end of a piece of wood on applying our ear to the other. Immediately on this suggestion, I rolled a quire of paper into a kind of cylinder, and applied one end of it to my patient's chest, and the other to my ear, and was not a little surprised and pleased to find that I could thereby perceive the action of the heart in a manner much more clear and distinct than I had ever been able to do by the immediate application of the ear. From this moment, I imagined that means might be found to ascertain the character, not merely of the action of the heart, but of every species of sound produced by the motion of all the organs within the chest."

Laennec was so convinced of the utility of his invention that he became impressed with the idea that he would be

enabled to discover "a set of new signs of diseases of the chest, simple and certain, and such as might probably render the diagnosis of these diseases as positive and circumstantial as those of many affections which come within the immediate reach of the hand or the instrument of the surgeon."

He scarcely over-rated the inestimable value of the stethoscope, and his conviction has to a very great extent been realised, for, by the aid of this instrument, a clear, steady and *certain* light has been thrown upon almost all the diseases to which the organs contained in the chest are liable.

The importance of an early discovery of the first onset of that fatal disease, "consumption," is well known, but anterior to the time of Laennec, premonitory warnings were not perceptible to the physician, and the unfortunate patient drifted slowly and surely into irremediable lung disease. But now, by auscultation, the earliest signs of phthisis are immediately recognised, and prompt treatment is frequently rewarded by the arrest of the fatal march of the disease. Many thousands of people have had their lives prolonged by Laennec's invention of the stethoscope, and thus phthisis justly considered the scourge of our climate, and answerable for a ninth part of the annual mortality, is becoming less fatal. Early and prompt treatment of disease is an essential necessity for success; correct treatment must depend upon accurately appreciating signs and symptoms, and this will explain to us how it is that we have been more successful in our treatment of phthisis since the time when the stethoscope first revealed to us all the minute morbid sounds which the unassisted ear could never, by any possibility, have heard.

Thus, the simplest method of investigation, unaccompanied by revolting torture, has been the means of making many a parent unconsciously bless the name of Laennec. Children, who formerly would have been considered doomed, have been

by his means snatched from an early grave and preserved for a useful life. Has vivisection anything so noble as this to boast of? If its ponderous annals be searched through, they will be found to be blank, whilst, on the other hand, the simplest principle in acoustics has, in the hands of a reflective man, led to the amelioration of a disease hitherto pitiless and unrestrainable. Well might the accomplished M. Bertin exclaim, in reference to the discovery of auscultation, "It has, in a few years, more completely illumined the diagnosis of the diseases in question (those of the chest), *than all the other modes of exploration have done for two centuries.*"

John Hunter's new method for the cure of aneurism has been upheld as another proof of the value of vivisection; but if we look into the facts, we cannot help seeing that, so far from this being the case, vivisection had absolutely nothing to do with it. The old method of operation was to open the tumour and tie the artery at each end; but this procedure was so eminently unsuccessful, that Hunter imagined that, as the artery was diseased at the seat of ligature, no proper reparative process could take place. He therefore suggested that the artery should be tied farther from the aneurismal sac, where there would be a fair chance of its being sound. This theory was amply confirmed by *post mortem* examinations on those who died of the disease, and sufficient proof was thus obtained that the affection does not necessarily extend far up the arterial tube. In opposition to this, several maintained that in aneurism, not only one, but all the arteries of the body are affected. Although such a state as an aneurismal diathesis may exist, yet in the great majority of cases only a portion of an artery is diseased, and that generally to a very limited extent. Truth ultimately prevailed, and Hunter's theory, established by dissection of the dead body, asserted its superiority, and thus one of the grandest triumphs of modern surgery was achieved.

The first time that this theory was put into practice was

upon the human subject, and not upon animals. The case in question was one of popliteal aneurism of such extent as to reach to the tendon of the triceps muscle, thus leaving no room for the application of a ligature between the aneurismal sac and the tendon. Hunter, therefore, seeing that the usual method of operation was inadmissible, had but two courses open for him to follow, either to amputate the limb or to tie the femoral artery. Rather than adopt the former alternative, he determined to make the experiment of tying the artery. The first operation was not, as might be expected, performed in the very best manner. He was not content with a single ligature, but applied several which were left hanging out of the wound; but, notwithstanding these disadvantages, the patient recovered. Sir Astley Cooper, in his "Lectures on Surgery," asserts that it was this successful case which induced Hunter to perform his experiments on the lower animals. He first applied his splendid anatomical knowledge to save human life with success, and then marred his triumph by repeating over and over again the same operation on animals. After the completion of this vivisection he tells the world:—"I have frequently tied the femoral artery in animals, without injury; why should not I put a ligature on the artery, in the same way, in the human subject?" It is palpable that, as he had already done this with complete success in the human subject, the question was needless, and the experiments were as unnecessary as cruel. If his object was to see what effect the ligature had upon the femoral artery, or by what means the collateral circulation was carried on, he could have ascertained this upon the human body, for the patient upon whom he first operated by the new method died about a year after. The body was examined, and it was found that the femoral artery was completely obliterated up to its division into the profunda femoris, and the method by which the blood was carried to the parts below the ligature could be made out by careful dissection.

Upon what grounds vivisectors can claim this operation as a result of experimental cruelty, one must be rather misty about, for nothing could be easier than to discover in the dead body the evidence of the extent of disease; the necessity of placing the ligature upon the sound part of the artery would follow as a natural consequence. The only difficulty consisted in finding a suitable place for exposing the artery; and this Hunter did, in the case of popliteal aneurism before-mentioned, in the canal called after him; but this locality has been altered for one still further away from the seat of disease, and still more convenient, in Scarpa's triangle. The improvements which have been made in the operative treatment of this disease are therefore entirely due to the inferences drawn from clinical observations, *post-mortem* examinations, and dissection. Any vivisection operations that Hunter was concerned in, relating to aneurism, were performed after he had invented his new operation, and they were probably then only carried out on account of the incredulity of some of his opponents, who required ocular proof of what, had they carefully studied morbid anatomy, would have been perfectly clear to them without the necessity of sacrificing several wretched animals. It was the gross carelessness of the earlier physiologists in overlooking important pathological and morbid facts that led so many men to take up vivisection, but with the acute and intellectual zeal of the present day, the last excuse for the practice of vivisecting animals has disappeared. Having shown how humanity has been benefited, and the duration of life increased by simple observation and research into nature, the other side of the picture reveals, as the result of vivisection experiments, an increase of suffering to humanity. One of the many serious errors resulting from vivisection was Sir Astley Cooper's method of treating fracture of the neck of the thigh bone within the capsule; and had it not been for the knowledge gained by *post-mortem* examinations, the effects of his erroneous conclusions would probably be visible

at the present day amongst those who have been unfortunate enough to have met with this accident.

Sir Astley Cooper laid it down as a general principle, that in these fractures "ossific union is not produced." In his lectures on surgery, he cited two preparations of Mr. Stanley, supposed to be specimens of bony union, but which, he assured his hearers, were nothing of the kind. He then mentioned his experiments made on animals by breaking the thigh bone within the capsule, and stated that he invariably found that the union was ligamentous and not ossific! His views, however, met with much opposition, notably from Abernethy and Baron Larrey; but to every case of bony union brought forward in opposition to his theory, he had the same answer, that "the fracture must have occurred externally to the capsule." Now, undoubtedly, Sir Astley Cooper's observations on the dogs were correct, as the difficulty of keeping the fractured ends of the bone in apposition, and the parts at perfect rest must, in animals, be enormous, if not absolutely impossible; but his application of the results of these experiments on dogs to treatment in man was most disastrous. This treatment is best described in his own words:—"Numerous measures have been adopted for the purpose of producing an ossific union of this fracture, both by myself and others, but all to no purpose. Disappointed in the attempt, and finding the patient's health suffer from the necessary confinement, what I now direct to be done is, that a pillow be placed under the limb, throughout the whole length, and another be put under the knee, and the limb in this way extended *for ten days or a fortnight*, until inflammation has subsided. Then let the patient get out of bed, and sit on a high chair, &c." The result of such treatment as permitting the patient to get out of bed "as soon as inflammation has subsided" is to prevent any chance there might have been of ossific union, consequently the bones can only unite by ligament, and the patient is irretrievably lamed for life.

The cases, considered as intracapsular fractures, which were brought against this treatment by eminent surgeons, were characterised by Sir Astley as extracapsular fractures; but this ought to have taught him the extreme difficulty of diagnosing the one from the other, and therefore to have caused him to give his patients the benefit of the doubt, rather than run the risk of permanently laming them.

After the lapse of time, opportunities of examining the bodies of those who had undoubtedly had intracapsular fracture occurred, and then it was proved conclusively that the experiments of Sir Astley had led him into error, since these fractures generally united by bone, the exceptions being cases of old people in whom vitality is at so low an ebb that no reparative process whatever takes place.

The result of these *post mortem* examinations has been that these fractures are now treated in the same way as those occurring externally to the capsule, and it is only in very old people, in whom there is no chance of union taking place, that Sir Astley's plan can ever be adopted.

Let us once more contrast the disastrous effects of applying vivisection conclusions to man with the results of acute clinical observation in the hands of a surgeon unstained by cruelty to the lower animals.

The constitutional disturbance and grave danger resulting from the bursting of a lumbar abscess is well-known; but the shrewd sagacity of Abernethy enunciated a mode of treatment that has been the means of saving many valuable lives. He observed in lumbar abscesses opened by caustic, that after the eschar was partially detached, and the contents of the abscess partly evacuated, the sides of the sac collapsed, closed up the opening, and that none of the dreaded constitutional symptoms followed; but when the eschar finally separated and exposed the interior walls of the sac, then, within a short period, all the worst symptoms supervened. Observations of this description showed him the value of excluding air from the interior of the abscess sac on account

of its tendency to set up putrefactive decomposition. He consequently taught that lumbar abscess should be opened before the skin was involved in the inflammation, or should this have unfortunately happened, then a valvular incision should be made on any other part but the inflamed integument, thus avoiding the risk of the weakened skin ulcerating and enlarging the puncture, and thereby admitting air to the interior. This plan insured the speedy healing of the wound with but very slight constitutional disturbance, and its adoption has prevented, in many cases, the usual terrible results.

Sir Astley Cooper made use of part only of this plan of treatment, though he stated that "the plan of Mr. Abernethy is the best that has ever been suggested by any surgeon." Contrary to the very essence of Abernethy's method, he said, "Let the abscess proceed until you observe a redness or blush of the skin, and then make a valvular incision." Mr. Abernethy very properly insisted upon the constitutional origin of this disease, and showed the necessity of strict attention to the general health.

Since the adoption, therefore, of this treatment, lumbar abscess has ceased to destroy so many lives as formerly, and the addition to it of the antiseptic method has produced marked benefit.

Simple observation has effected all this good for mankind, and in these *real* improvements, no cruelty has marred the God-like science of medicine and surgery; Abernethy himself, one of the most eminent surgeons the world has produced, abhorred with all his soul the very idea of vivisection.

The present results of ovariotomy are claimed as emanating from vivisection, the operation having been perfected by experiments on rabbits and guinea pigs! Undoubtedly, for a long time, this operation was in its results anything but encouraging to the surgeon, and so high was professional feeling against it that the late Baker Brown was threatened by his colleagues with an inquest on his next fatal case!

But it would, at the present time, be difficult to find a surgeon who does not consider the operation to be a legitimate one. The attempt, however, to point out this success as due to vivisection is manifestly unfair, seeing that the lessening of the mortality after operation did not happen *immediately* after the experiments on the rabbits and guinea pigs, but was *gradual*, as the following statistics of Mr. Spencer Wells show:—"In the first five years, about one in three died; in the second and third five years, about one in four died; in the fourth five years, about one in five died; but in the last two years, only about one in ten died!" From these data, it is evident that the diminution of mortality has been gradual, whereas, had the knowledge obtained by vivisection been true, we ought naturally to have expected to have had an *immediate and rapid fall* in the death-rate. The first mortality is somewhere about 34 per cent., the present is about 10 per cent.; and it has taken over twenty years to produce this satisfactory result!

Ovariotomists acknowledge that increasing experience has contributed very largely towards lowering the heavy mortality; mechanical dexterity has been acquired, and the improved methods of diagnosis have served to facilitate the operation.

Mr. Spencer Wells, in his *first* ovariotomy case, relates (*British Medical Journal*, December, 1877) how on opening the peritoneum, the tumour was proved beyond all doubt to be behind the intestines, and he was therefore induced to close the wound and do nothing more. The patient recovered from the operation, but died four months afterwards in St. Bartholomew's Hospital of some other disease, when it was found that it was a tumour of the left ovary, "which could have been removed easily."

One cannot help believing that with a vastly increased experience, surgeons would not be led into such an error of diagnosis again. At that time, the operation was in its infancy, and everything concerning it had to be learned by

experience, as Mr. Spencer Wells himself points out:—"I could occupy far more time than you could spare if I were to go over in detail the cases of successive years, tell you how each step of the operation has been *gradually* modified, how all the details of the *nursing* and *after-treatment* have been gradually brought to their present stage."

In the careful after-treatment of this operation lies the chief cause of the decreasing mortality. It was found at one time that the numerous visitors to the Samaritan Hospital brought with them disease germs which poisoned the patients; hence the necessity of each visitor signing a declaration that he had not been near an infectious case during the previous seven days was apparent, and no doubt has tended to lessen the risk of peritonitis. The use of carbolic acid dressings, and more recently of thymol, have been the means of warding off hyperpyrexia or even septicæmia. These are some of the means which have contributed to establish this operation upon a sound basis, notwithstanding the opposition it had to encounter in its early days. Vivisection has had nothing to do with it, though it attempted to show that the cutting into the peritoneum was not such a formidable operation as had been supposed; but this proceeding was useless, for wounds of this membrane are common enough, and many surgeons, such as Dr. Blundell, have shown years ago that the extreme fear which operators had of wounding the peritoneum was groundless. All that Mr. Spencer Wells himself claims as the result of his experiments on the rabbits and guinea pigs is that he ascertained "the best way of closing wounds of the abdomen whether made in ovariotomy or some other surgical operation, or by wound or injury." The lesson that claims to be derived from his vivisection experiments is, that the sides of the cut peritoneum should be placed in apposition and stitched together.

Now, according to the *Lancet* (September 29th, 1877), the animals used for these experiments "enjoy immunity from

severe peritonitis," hence it is open to doubt as to whether the results obtained could be made applicable to human beings, who are eminently liable to this form of inflammation; indeed, what may be true of a rabbit or guinea pig cannot be assumed to be true also of man.

Many celebrated ovariotomists have not hitherto seemed disposed to adopt Mr. Wells' method, and Mr. Hime, in his "Remarks on Ovariotomy," says:—"It cannot be so important to include the peritoneum in the abdominal sutures as Mr. Wells supposes, seeing that Koeberle and other successful ovariotomists have never done so, and it must generally happen when it is so included that it will double up into the wound more or less, and prevent union throughout the entire thickness of the sides of the wound."

In addition to this, Mr. Holmes, in his magnificent "System of Surgery," remarks, "Mr. Spencer Wells' practice has shown that it is safe to pass the needles through the peritoneum, but it may be doubted if any material advantage is obtained."

Finally, to confirm the argument that these experiments were of no practical utility, we find that in Mr. Wells' first hundred cases of operation immediately after those vivisection experiments, the mortality was 34 per cent., whereas in all the *previous* cases that could be collected, the mortality was only 33.89 per cent.,—a slight difference, it is true, but still sufficient to demonstrate the fact that no practical improvement in the operation resulted from the pain and mutilation suffered by the rabbits and guinea pigs.

Singularly enough, the practice of including the peritoneum in the sutures uniting the abdominal wound, seems to have given rise to a discussion as to whom the original discovery is due. Dr. Clay, of Manchester, claims to have adopted this plan prior to Mr. Wells, for he says,—"This has always been my practice since my first case in 1842, fifteen years before Mr. Wells ever operated;" and he further states that "this plan of sutures was arrived at *without any experiments on living animals.*" It is satisfactory to feel that,

whether this method of operation be useful or not, its origin was not due to vivisection; and considering the many diverse opinions as to its utility, should it ever in the future be possible that it should be recognised as a means of saving life, humane people may be able to rejoice that, far from its being an outcome of cruelty, it is one of the triumphs of legitimate research.

The conclusions to which we are forced with respect to this question are, that statistics point to a very slight increase, at first, of mortality after the new plan was adopted; that the mortality fell afterwards during a period of twenty years, this being due to increased experience in the after-treatment; and finally, assuming that the new method has been so beneficial, we have yet to be convinced whether it was the result of vivisection, or whether, as Dr. Clay asserts, it was discovered without causing agony to any animal.

CHAPTER III.

It is a truth which cannot be gainsayed, that the mortality from disease has been perceptibly decreasing for many years past, and that, in many instances, disease itself is of a much less virulent type than formerly. Causes must have been long at work to produce these effects, and no doubt vivisection claims her share in this good work. In the Mosaic law, we find it laid down that health follows as a natural sequence of morality, and, as far as can be ascertained, physiological laws assimilate very closely to moral laws, and this gives us the clue to the intimate relationship between the science of health and religion. But the question may be asked, how can physiology, based upon infringements of the moral law, having its foundations laid in the awful agonies of the brute creation, be considered a God-like science? Fortunately, for morality and religion, physiological knowledge, properly so-called, has been raised up independently of any such terrible foundations; were it otherwise, faith, both in morality and religion, would receive a rude shock. A cruel science could not corroborate the wisdom of the Creator any more than the fiendish orgies of Moloch could show the mercies of this heathen deity. Happily, then, the morality or physiology of the Bible requires no cruelty for its demonstration; and if we take a lesson out of that Book, we cannot fail to see that the torture of any sentient being, for any purpose whatever, is utterly repugnant to the infinite mercy of the Creator.

In inquiring into the causes which have tended to diminish disease, we may therefore exclude vivisection, on the ground that nothing immoral can produce any permanent good, as has been amply demonstrated in the preceding pages.

In the first place, increase of morality has been an important factor in diminishing disease. As education advanced,

and the people ceased to be plunged in the lowest depths of superstitious ignorance, new ideas, higher aspirations, and a better appreciation of humanity arose. One of the earliest signs of the approach of a more moral era was the gradual abolition of the torture and its hideous accompaniments as legal punishments; executions became less frequent, and the criminal code was rendered more merciful. Pauperism and crime are far less wide-spread than they were thirty years ago, whilst the prosperity of all classes has enormously increased during this period. This increase of morality has consequently brought its physiological increment of health, which means a larger capacity both for work and enjoyment, and a diminution of those diseases which are caused by infractions of the moral law. Again, chemistry has played an important part in lessening disease. With the discovery of oxygen, physiology was almost revolutionized; we then understood the theory of respiration aright for the first time, as well as the changes the blood undergoes, and the union of oxygen with the tissues; in short, all the manifold workings of the human organism. If ever anything has placed physiology in its present useful position, it is the science of the chemist. To him we are indebted a thousand-fold; to the vivisector we are indebted for the retardation of knowledge, and for the errors which have resulted in the sacrifice of many precious lives.

The real treatment of disease (to use an anachronism) consists in its prevention, and not in the application of erroneous physiological laws to the patient. Upon physiological principles, huge quantities of alcohol used to be prescribed for patients in the adynamic stage of fevers. Clinical observation has induced many eminent physicians to regard this practice as erroneous and dangerous. But in the prevention of disease, chemistry comes to our rescue and gives us considerable knowledge of the various infectious malaria and the means we have of destroying these disease agencies; it has also instructed us upon the all-important

sewage question, and shown the necessity of good drainage. Thus, experimental physiology is relegated to the background, and the science of chemistry has commenced to effect such a reduction in disease, by its prevention, that it soars like a giant far above the puny efforts of vivisection. Chemistry practically took its rise at the beginning of this century—vivisection has been in vogue for more than two thousand years; yet can the latter, with all the odour of antiquity about it, point to such grand results and benefactions as the former has achieved for mankind? Apart from the power the chemist has given us of destroying the infectious properties of disease germs, and thus preventing their propagation to numerous individuals, we have also the antiseptic treatment, which promises to save many valuable lives. These few remarks show that chemistry has done more good for humanity than vivisection, and if we go through the list of other sciences, we shall still see that they have all helped, whilst vivisection has retarded medical science.

The rapid increase of population has necessitated the bringing under cultivation a vast area of waste land, marshes, and woods; with the gradual progress of this drainage and clearing, ague, formerly very common in England, has now completely disappeared. The extinction of this disease has been effected by agricultural science. Has vivisection ever extinguished a disease? In the times when even the most wealthy had to live during the winter months almost exclusively upon meat (very often salted), bread and flour puddings, and when the diet, therefore, was far too highly azotised and deficient in fresh vegetables, arthritic, calculous, and scorbutic disorders were much more common than at present. But as agriculture advanced, and it was found possible to keep a supply of fresh meat and vegetables during the winter months, particularly the potato, then these diseases declined. In many disorders at the present time, owing to this alteration of diet, there is

a greater tendency to favourable terminations than used to obtain formerly. Agricultural science has been therefore an important helpmate to medicine, and together with other collateral sciences, has been eminently productive of health to the community, whilst so-called physiological laws, based upon vivisection experiments, have been utterly barren.

These are a few amongst the many reasons that can be brought forward to substantiate the fact that, whilst other sciences have initiated and carried out methods of reducing and extinguishing disease, vivisection is still plodding on, barren yet of any benefit to mankind, and only holding out promises for the future which can never be realised. If we judge vivisection by its fruits, our judgement must be adverse; it has had a long trial, extending over many hundreds of years; it has raised theories based upon unutterable tortures; it has overthrown these theories by similar methods, and so it has gone on for centuries, elevating structures simply on the sand, no foundation being obtainable on the rock, the edifice being reared of fallacies.

Such being the case, is it to be wondered at that humane men at last step in and endeavour to end all this horrible torture? The cry against vivisection does not rest alone upon what some are pleased to term "sentimentalism,' although if that term means a horror of producing pain or a horror of seeing it in others, then no one need be ashamed of having it hurled at him. The outcry against these experiments, independently of the moral law, is based upon their uselessness as evidenced by criticism of the results, upon the fact of the order of nature being inverted, and upon the total want of a true philosophic procedure.

CHAPTER IV.

It has now been shown that, from a practical point of view, vivisection experiments have failed in their object of doing good to mankind. It is, of course, thoroughly understood that these operations which are so constantly taking place, cannot be performed without causing the most excruciating torments to the poor animal, whose manifestations of distress and agony are plainly visible to the operator, but who, in the interests of science, seems bound to disregard them. The physiologist is, therefore, daily brought into contact with acute exhibitions of suffering produced by himself, and consequently it may be useful to inquire into the mental state produced by this *habitual* disregard of pain inflicted on others.

Habit may be said to consist in the frequent repetition of certain acts which tend, after a time, to weaken the sensibility both of the mind and the bodily organs. It is a matter of every-day remark that substances which used to excite pleasure by the gentle stimulus they afforded, lose that power after a time; that is to say, the stimulus by frequent use has lost its force; it can neither excite the bodily sensations, nor cause a mental excitement. The use of snuff first increases the secretion of mucus in the nose, but after a time ceases to affect the pituitary membrane unless the quantity be greatly increased. In the same way, alcohol, by constant use, loses its power of exciting the sensibility of the body. It is therefore evident that frequent and oft-repeated stimulation of any organ results in the blunting of its sensibility. Now, every physical act has its mental accompaniment. In the illustrations above-cited, the mental perception of pleasure would be annihilated unless increased

stimulation be resorted to, with the manifest discharge of an increased amount of nerve-force, resulting in gradual exhaustion. Hence, after the frequent repetition of a certain act, the mind as well as the body has undergone a blunting operation, healthy sensation and perception being diminished or ultimately lost; all that remains is the idea that such or such a substance formerly contributed to bodily pleasure or happiness. This, carried to its greatest extent, is the triumph of ennui as applicable to that weariness of mind and body produced by the exhaustion of all pleasures and enjoyments. One of the mental peculiarities of habit consists in the perpetration of the habitual act (which perhaps was at first acquired with difficulty and by constant efforts of the will) without rousing the consciousness. For example, a smoker first experiences difficulties in acquiring the practice, nausea perhaps; to keep his cigar alight, requires attention, but after long practice the habit becomes confirmed, and the smoker may perform many other acts without any conscious effort on his part to keep the cigar alight, and without any conscious knowledge that he is smoking. What is true of one habitual act is true of the whole class; if once the habit of doing good is acquired, it is then much easier to be virtuous than to do evil; and on the other hand, if vice becomes habitual, it is terribly difficult to throw it off.

It is evident, therefore, that to become the slave of habit blunts the physical and mental processes, and weakens the power of the will. If the habit be a vicious one, as it becomes more and more confirmed, the will has less and less power for good, and at last the faculty called "moral sense," or the power whereby we discern between right and wrong, becomes thoroughly blunted. Men often take up some particular branch of study, and as their interests deepen in the subject, it engrosses the whole thought and intellectual powers, to the utter exclusion of everything else. Thus, a habit is formed in a direction which not unfrequently verges

upon monomania, and so intense does it become, that the normal operations of reason and conscience seem suspended. The collector of coins stealing rareties from his friend's collection; the priest robbing a traveller to obtain money in order that he might purchase books necessary to help his solution of some problem of theological casuistry, are cases in point. The most blameless man, austere in his morals, is thus led to the commission of crime in his monomaniacal thirst after knowledge. Granted that these cases are rare, yet, though they may lie at the extreme end of the series, there are gradations of crime and perversions of moral sense which form, as it were, steps from the lowest rung of the ladder up to the highest. If, then, the will is made a slave to habit, and the moral sense is thus perverted, what becomes of the noblest and holiest feeling of human nature—sympathy?

The ordinary action of the will is to gain our own pleasures and remove our own pains. Each organism seems to have been more or less formed for conserving itself, and any action of the will out of this sphere would seem irrelevant; therefore, if we were to look at it from this animal point of view, fellow-feeling, pity, compassion, disinterestedness, self-sacrifice, and patriotism ought to have no existence.

But the fact of there being such names in language expressing these abstract qualities, and the experience that most people have had of their operations, proves their existence as mental phenomena. "We are able to conceive the pains of other beings by our experience of the like, and when we do conceive them, we feel urged to the same steps of alleviation as if the pains were our own. . . . . To see a person cold and hungry is to take on the idea of these painful states, and we are induced by the power of the idea to relieve the pain that occasioned it" ("Senses and Intellect." —*Bain*). This is sympathy, which is a mighty lever for the good of all sentient beings; it limits the terrible degradation and suffering under which creation groans. Never

has it been more powerfully exhibited than when our Saviour was on this earth, and by His intense sympathy raised the dead and healed all manner of sicknesses.

Perhaps sympathy cannot be entirely denied to the vivisector. To the outside world, he is the calm, humane man; arouse his attention to a case of distress, he will hasten to relieve it; but, in his own particular department, such is the force of habit, such is the intense concentration of thought upon the experiment, that the animal is regarded more as a vivified machine capable of imparting knowledge, than as a sentient being, possessing sensitive nerves rendering it amenable to the perception of the smallest as well as the most exquisite pain. It is quite possible to conceive the abstraction of the mind from all extraneous considerations, such as pain, when the whole power of thought is concentrated upon *one idea only*—the experiment. No sympathy can possibly be shown to anyone or anything that interferes with the progress of the operation; if the cries of the animal disturb the operator, it is silenced by blows; if it move, it is fastened in a position more secure and more torturing than previously.

Habit, therefore, tends to deaden sympathy in this case; but constant repetition must ultimately lower the whole moral sense. From simply being callous to the agonies of animals in the laboratory, the habit becomes applied to the whole class of animal creation, and a total indifference to suffering is the ultimate and inevitable result. A fatal facility towards the reproduction of any kind of false or wrong ideas is engendered by habit, and when once formed they can never be eradicated. The only chance of antagonizing their baneful influence is by attempting to accumulate stronger tendencies of an opposite character. The great danger in forming a habit of cruelty, no matter for what purpose, consists in its blunting effects upon the higher emotional faculties of the mind. These are the faculties whereby we discern right from wrong; whence we derive

our ideas of religion, of faith, and of love and charity towards all.

The importance, therefore, of not allowing these to be invaded by the lower sensuous faculties is apparent. Vivisection is essentially a selfish proceeding, prompted solely by the lower faculties, which urge men to commit all sorts of crimes for self-preservation. Self-preservation may be one of the first laws of Nature in the brute creation, but man's higher faculties teach him that it ought not always to be the rule with him. If our Saviour, as a man, had carried out this law, where would our salvation be? The selfish idea involved in vivisection, the torturing of animals in order that man may escape from pain, is utterly repugnant to those higher ideas and nobler faculties that elevate man above the level of the brute.

It may be objected to the foregoing argument, that it would apply just as well to the surgeon as to the vivisector; for the former, by the constant practice of operating, will, by exactly the same process, ultimately become callous to suffering, and have the moral sense blunted.

An objection of this kind is specious, but easily controverted.

A man's moral sense can only be blunted by the accumulation of vicious tendencies; and, therefore, when he considers the probable moral result of an action, he must carefully estimate what influence the perpetration of the act may have upon himself and upon his surroundings; and it may be laid down as an axiom, that no action can be good which, being the cause of pain and suffering, violates a moral law.

The surgeon has for his aim the relief of human suffering, and although the idea of inflicting pain is repugnant to a right-minded man, yet, when firmly convinced that the operation will either cure or alleviate disease, then the result wished for is beneficent, and the proceeding is not condemned by morality. The greatest and truest argument

in favour of the surgeon is, that his art is but the copy of the natural processes of nature, whereby a dead or diseased part is extruded and a cure effected. The same plan is adopted that nature herself uses, with but this difference, that surgical art is more expeditious than the natural efforts, and hence the surgeon really saves the patient a vast amount of suffering by hastening the reparative processes. Surgery is a noble science, upheld by all morality; it does not outrage nature, but assists her, and therefore no shadow of a doubt can be cast upon its beneficial character. Its practice, then, does not cause its professors to accumulate evil tendencies; they follow closely the laws of nature—that is, the laws of God; hence it tends to elevate the mind, by bringing it into closer contact with the Creator through His works.

The vivisector avowedly starts with the same object as the surgeon—the relief of suffering humanity. In his endeavour to accomplish this, he has to make use of an *intermediate agent*—the brute creation. Without doubt the object he has in view is a good and legitimate one; but the question is, as to man's right to make use of this intermediate agent by the infliction of horrible cruelty. It has been shown that the surgeon's method is permissible, but the vivisector adopts the very *reverse* plan; he destroys health by terrible pain, and causes death—a process contrary to the creative laws. The surgeon restores to health and saves life, this being in accordance with all moral and Divine law.

The supposition that some benefit may possibly accrue to mankind by experimenting upon animals is no sufficient warrant for so utterly reversing the plain order and harmony of nature. The superstitious heathen were accustomed to consult the entrails of animals to obtain an augury, believing that they could thus get forebodings of good or evil; in a precisely similar manner, the superstition of science imagines it possible, by outraging natural laws, by deranging

healthy functions, and by upsetting the whole equilibrium of the body, to arrive at the laws of health!

Nothing ought to be more self-evident than that the "doing of evil in order that good may come of it" bears upon it the impress of distorted moral sense; no good, morally speaking, ever has or ever will be derived from evil means—that is to say, by means we would be ashamed to apply in every-day life. The evil simply recoils upon the perpetrator by deadening sympathy and fostering a habit which stifles the higher moral faculties, reducing the promptings of conscience to a most undesirable minimum. It is impossible to say whether any experimenting physiologist ever has arrived at such a low moral condition, but all the facts of psychology tend to show that this is the legitimate conclusion to be arrived at. The late Mr. Abernethy evidently had seen that vivisection could produce such a peculiar moral state, for he thus publicly stigmatized the vivisector Spalanzani:—"I believe Spalanzani to have been one of those who have tortured and destroyed animals in vain. . . . . Besides, some of his experiments are of a nature that a good man would have blushed to think of, and a wise man ashamed to publish, for they prove no fact requiring to be proved, and only show that the aforesaid Abbé was a filthy-minded fellow."

It is encouraging that one of the most eminent surgeons of this century should thus boldly, and at a time when vivisection was at its infamous zenith, speak out his mind, and in the following extract from one of his lectures, he leaves the particular and becomes more general:—"But I know that these experiments tend to *harden the feelings*, which often lead to the inconsiderate performance of them. . . . . I, at the same time, express an earnest hope that the character of an English surgeon may never be tarnished by the commission of inconsiderate or unnecessary cruelty."

CHAPTER V.

In support of the practice of vivisection, Nature herself has been appealed to, to prove that in performing scientific tortures, the operators are doing nothing more than she herself is daily and hourly accomplishing. Those who uphold vivisection assert that we cannot attempt to be wiser than Nature, who, though cruel, possesses the power of evolving good out of her evil processes! This is the argument drawn from Nature by vivisectors, but its fallacy, and its flimsy transparency must be apparent to the most shallow mind.

However, as the laws of Nature have been thus appealed to in support of a cruel cause, and the endeavour has thus been made to drag down the mercy and justice of the Creator to the level of pitiless injustice and cruelty, it will be advisable to show that Nature, far from being cruel, is beneficent, and that the Creator originally established a perfect harmony throughout the whole universe.

No study can more thoroughly imbue one with the idea of the necessity of harmony for all creation than that of astronomy. We must be convinced by the spectacle revealed to us of mighty worlds revolving along their accustomed paths; by the unvarying action of various forces through incommensurable centuries; and we know that were one of these forces suspended but for an instant, huge planets would be hurled in chaos against each other. The study of this science alone ought to be sufficient to show us that harmony is the great law of the universe. But if this law holds good for an entirety, it surely must also be true for each of the parts composing the whole, and therefore it is impossible to elimi-

nate it from the processes that are constantly going on, on this earth.

It can be safely affirmed, that for a lengthened period after the creation of man, all nature worked together in perfect harmony; then came the Fall, with all its direful consequences, as the result of sin. How this latter came into the world, no philosophy can tell us; but as it is impossible to imagine that original righteousness could have been unfolded into original sin, so we must assume that "sin was a new thing, originated *de nihilo* by the Finite Will" *(Shedd)*.

The loss of original innocence and purity gradually caused the retrogression of man physically and mentally. As evil mental experience grew and re-acted upon the body, disease became possible, and death the heritage of all. As at this day, the power of mind over body is recognised as an agency in altering the functions, so in those long-distant ages, the mind was the prime cause of the deterioration of bodily structure.

If we believe that most ancient of books, the Bible, it is evident that the brute creation enjoyed with man the beauties of harmony, and suffered, through man, after the Fall. But even independent of the Bible, the theory of a pre-existing harmony forces its truth upon us by a consciousness of that desire for peace which all men must have experienced. The most enjoyable parts of a man's life are those quiet, peaceful days of rest snatched for a brief period from the busy turmoil of the world. Our Reason leads us to long for and aspire after an harmonious and peaceful life; and its highest exaltation has for its object the discovery of those laws which, once in our possession, would gradually reduce that discord in which we live with nature, and enable us once more to live harmoniously with the rest of the universe. It is man who, by his sin, has so altered his bodily structure and lowered his vital force that is to blame, for he originally upset that perfect equilibrium existing between himself and the natural forces around, and consequently

lost the power of regulating his life so as to harmonize with the physical forces. The result is, that sooner or later, nature asserts her sway, and disease and death become inevitable.

The forces of nature which thus destroy man's physical body are the same with those which regulate the universe, the motions of the planets, the structure of the earth, and all the varied phenomena we see around us.

Can nature be considered cruel when we see that she has remained unchanged from the beginning, whilst man, by his own act, has detached himself from her harmonious operations? Man lives by a power of reaction against the physical forces; through his vital power, he is enabled to withstand the antagonistic forces of nature, and establish some species of harmony. But it must be borne in mind that ultimately the physical forces triumph, and the body becomes merged again into its primal elements, thus showing that there is not a perfect equilibrium established, and that the vital power must succumb—that is, the body must die.

It is easily conceivable, then, that previous to the Fall, the harmony was perfect, and that whilst man strictly obeyed the laws of his being, death was impossible, the opposing forces being so accurately balanced as to neutralize each other in any harmful tendencies, and thus maintaining an absolute health.

If we glance at what the state of man living thus harmoniously with nature would be, we cannot help believing that *pain* would be an impossibility.

Dr. Barnes defines physiology to consist in "the perfect adjustment of organs working harmoniously to the accomplishment of natural functions" (*British Medical Journal*, December, 1876). As is elsewhere shown, pain is an important factor in causing functional disorder; but upon the hypothesis of "perfect harmony," disturbance of function could not exist; and, therefore, we can safely conclude that pain could not exist in a being so constituted.

But, at this period, the human race formed a very small part of creation. The lower animals, living in a world so accurately adjusted, must necessarily have enjoyed the same immunity from pain and death as man himself, upon the principle of their forming a part of one harmonious whole. This was the perfection of our first parents, and of the whole of the animated creation, and the retrogression from this state brought the woes and miseries under which all groan alike. This, then, ought to be a plea against the useless perpetration of pain and cruelty on the lower animals. Through man's superior intellectual force they became partakers of the Fall, and from their present treatment it would seem as if the curse had alighted doubly upon them.

What is the object of man's life in this world? It certainly is not that he may clothe his body in purple and fine linen, and gratify his lower sensuous faculties, and, in case of disease brought on through this selfish gratification, attempt to cure himself by the infliction of pain upon poor dumb animals. His aim is to prepare himself for a state similar to that of our first parents, who, while still innocent, enjoyed close communion with the Creator; it is to attempt to regain his lost purity, by putting aside all selfishness, and by keeping the lower faculties under the resolute guidance of a pure reason and strong will.

The infliction of torture cannot be a means of bringing this to pass, for it is condemned by everyone whose moral sense is not entirely obliterated. The aim and object of the Christian is to restore harmony to the world; true philosophy is an aid to this endeavour; but can the same be said of a science which is founded upon a disturbance of the natural harmony by producing torturing pain, lingering disease, and an agonising death in animals? Is this the way that philosophers have of showing us how to attain the goal of all our hopes and aspirations? Is it by torturing animals, and thus rendering them our enemies and distrustful of us,

that we can at last hope to realise those prophetic words, that the wolf shall dwell with the lamb, and the leopard with the kid, that the child shall play on the hole of an asp, and put his hand on the cockatrice' den? (Isaiah, c. xi.) How can such a beautiful harmony, as Isaiah predicts, ever exist if the very means vivisectors take to ensure it are those which are bound most strenuously to oppose and destroy it? Through our sin and folly we have brought into the world a huge mass of disease, and by lessening it we have some hopes of being able to improve both our bodily and mental condition once more. But all physiological research teaches that morality is the prime factor in diminishing disease, or even the tendency to it, hence how can the immoral methods of inducing disease in animals and foully using their quivering bodies by the infliction of such pain that one would hope no human being ever suffered, do more than lead astray both morally and physically, and consequently retard that moral and intellectual development which ought ultimately to culminate in a restored harmony?

It may be objected, that animals never could have existed in the same freedom of happiness as man, "for," argue the objectors, "the possession by animals of limbs, jaws, and various appendages as weapons of offence and defence, is sufficient to prove an insurmountable obstacle to the theory of a pre-existing harmony; and had animals been intended to live in peace one with another, these undoubted instruments of attack would never have been formed."

The answer to this objection is, that these offensive organs did not exist as weapons of attack or defence during the period of "perfect harmony." Observation has taught us how certain parts, which have become useless in the economy, dwindle away, and on the other hand, how organs by constant use become enlarged and modified, being affected by the direct action of the external conditions of life. Constant habit produces marked varia-

tion in structure, and it therefore becomes easy to see how, in the course of time, after the commencing degeneracy of the world, an organ originally intended to subserve some function of the body, becomes modified so as also to be useful as a means of attack. Thus teeth, first of all designed for the prehension and mastication of food, were found to be also powerful weapons of offence; the sting of the bee, primarily an ovipositor, and the poison ejected from it serving merely to agglutinate the ova, becomes an instrument of attack. In addition to structure being thus modified by the necessities of the case, there are also good grounds for believing that secretion becomes altered after the lapse of time. The influence of passing fear, or any emotion, has been shown to temporarily affect secretion; but these emotions acting constantly in animals, when they are against each other, and man against them all, must have in time effected a permanent alteration of secretion; and probably, in the case of the bee, the poison ejected from the sting was originally an innocuous fluid.

These are examples of parts destined for the performance of natural functions becoming appropriated to the purposes of attack or defence; in other animals, special parts have been specially developed, in the course of time, for the required object.

The theory, that parts now used for attack were not originally designed for that object, receives a striking confirmation in the case of the bee. Paley has remarked that "no organ will be formed for the purpose of causing pain or for doing an injury to its possessor." But when we consider that the sting of the bee, when used against an enemy, cannot be withdrawn owing to its backward serratures, and thus causes the death of its possessor by the dragging out of the abdominal viscera, it seems, at first sight, as if Dr. Paley's conclusions were wrong. But we must not regard the sting as a weapon of offence, but only as an ovipositor; and thus, when the organ performs its *natural* functions, no

harm results, but when it is made to act in a manner alien to the intention of its original formation, fatal consequences ensue. If this organ had originally been formed for the purpose of attack, it would militate against the doctrine of pre-existing harmony, as well as against the Biblical account of Earth's original state of happiness.

It can then be affirmed that whatever weapons of offence animals now possess, have been produced from originally harmless structures, by increasing necessity both for warding off danger and obtaining prey; that, in some cases at least, when an organ is used to perform a duty alien to its natural function, death or injury ensues; that, considering the position of man, when pure, in an harmonious sphere, there can be no valid grounds for refusing to admit animals as partakers of the benefits of this harmony, and consequently, that offensive organs formed no part of their structure, but were developed subsequently; that the great and universal law of harmony, which governs the minutest speck equally with the mightiest planet, has been disturbed solely by man's wilful refusal to live in accordance with it; that the natural equilibrium has been destroyed by the altered conditions of human life, the vital force being now insufficient to counteract the physical forces of the universe.

Surely, on reflection, the injustice we have been guilty of towards the brute creation must be acknowledged. Not content with depriving animals of happiness, we have forced them to be our slaves, to minister to our wants, to supply us with clothing and with luxuries, and the return for all this faithful drudgery and loss of life in our service is practically a determined effort not to regard them as sentient beings, endowed with the faculties of appreciating pleasure and of being keenly alive to pain, but as mere machines suitable for cutting into and hacking about in order to elucidate some new (or often old) physiological problem.

If Atheistic morality exclaims against such an unjust return for all the benefits we owe to the lower animals, how much more therefore ought Christian morality to abhor such unfair dealings and such horrible torments! The mind of man is fertile in ingenuity, but one can hardly conceive it possible that any human being, possessed of the smallest atom of sympathy, could set his ingenuity to work to discover some of the most awful and hellish tortures which would disgrace a Nero, or a man ten-thousandfold more fiendish than he. No discovery, if any could be made by this method, however grand or useful, would justify such a flagrant breach of the fundamental law of our higher being —sympathy. It is by this law that we beget compassion, a power of feeling for the sorrows and pains of others, as if we ourselves suffered them. Let sympathy be deadened ever so little, we then begin to trace a downward step; there is a loss of something from the little excellence we possess, and which we can so ill afford to lose; repetition deadens it still more; a habit is formed, and the last state of that man is worse than the first. No work of destruction* was ever performed by that grandest of all philosophers, Christ; He exhibited the intensest sympathy with all creation, and performed the part of physician not only of the body but of the soul. Let him who would be a philosopher humbly follow His example and walk in His footsteps; let the true man of science remember that in seeking to discover laws of nature, he is penetrating the works of God. How humbly, then, and with what reverence ought he to approach his task, second only to that of direct worship itself; feeling, as he unravels the wondrous works of the Creator, that he is nearing Him. Into this temple of God, made holy by His all-pervading presence, ought any pain, cruelty, or worldly thought to be introduced? As he approaches this temple, let the physiologist cast away his cruel

* Except one, the cursing of the fig tree.

vivisections, and only gratefully remember, as evidence of the intense love and care bestowed upon *all* created things by their Creator, that not one sparrow falls to the ground without the knowledge of God.

The existence of a perfect harmony between all created things must convey the idea that man never, in this state, took the life of an animal either for the purpose of food or for any other reason.

In the debate on Lord Truro's Bill for the total abolition of vivisection, the Bishop of Peterborough is reported to have said:—"But what was food? Food was the means of prolonging human life. The killing of animals for the purpose of prolonging human life was as justifiable in the one case as in the other."

If the Creator had, from the beginning, given the lower animals over to man for food, the theory of an original harmony would vanish; but the 29th and 30th verses of the 1st chapter of Genesis are very explicit as to the nutriment the animal world is to live upon:—

"And God said, Behold, I have given you every herb bearing seed, which is upon the face of all the earth, and every tree which is the fruit of a tree yielding seed; *to you it shall be for meat.* And to every beast of the earth, and to every fowl of the air, and to everything that creepeth upon the earth, wherein there is life, *I have given every green herb for meat.*"

From this it appears that in the primal ages of the world a vegetable diet only was permitted; and the above quotation contains a silent prohibition from taking life under any pretext whatever. That food derived entirely from the vegetable kingdom is capable of supporting life in perfect vigour, is proved by the chemical discovery of the identity of animal and vegetable proteids or food-principles necessary for the due repair of the wasted tissues of the body. All these principles, essential to sustain the life of an animal, are found in the plant world; hence undoubtedly the vege-

tarian theory has the weight of argument on its side, and we are enabled to assert that the killing of animals for the purpose of food is unnecessary.

In our endeavours to progress towards perfection, and to "regain the blissful seat" now lost to us, we involuntarily cast the mental eye back through countless ages, until it dwells upon the lovely vision of primeval harmony; here the original scheme of creation seems unfolded to us; no pain or death lingers there, but all seems perfect happiness, perfect health. To upset this picture of a universal Eden, comes the doubt that the Creator sanctioned the death of his creatures by the institution of sacrificial rites. Is it, however, possible to believe that God, the same yesterday, to-day, and for ever, has ever altered the laws He first impressed upon the world and its inhabitants? Man, not God, departed from them, and as the ages rolled on, the sanctity of life was invaded with such recklessness, that the Creator from time to time interfered for the protection of his creation. The first mention of an animal being offered up for sacrifice is *after* the Fall, and it has been keenly debated whether sacrifice in primitive time was of divine or human origin. From the present standpoint, the idea of a Divine origin must be dismissed, while in favour of its being an emanation from the mind of man, it may be said that the human conscience so feels a load of guilt, and so forebodes an awful penalty of death, that it struggles to be freed from it by the substitution and slaughter of a living victim in its room. Regarding sacrifice, then, as an invention of man, we, however, see the hand of God protecting his creatures by the laying down of certain rules and ceremonies to be observed whenever an animal was to be offered up.

We find it distinctly asserted that if these regulations be not strictly conformed to, *blood will be imputed to the breaker of them.* God declared that "he delighted not in burnt-offerings," and it seems evident, on perusing Leviticus

(chapter xvii. ver. 3 and 4), that the taking the life of an animal is abhorrent to Him; for here it is stated, that if atonement is not made for the infraction of the rules, "he hath shed blood; and that man shall be cut off from among his people."

Let it be remembered that these stringent regulations were not made for those who *tortured* animals to death, but simply for these who killed, by an almost painless method, any of God's creation; how much more severely punished would the torturer have been! It is plain that the Mosaic ordinances very jealously guarded life in its sentient form, permitting no torture under any pretext whatever; then, with the atonement, Christ utterly abolished the sacrificial putting of animals to death; and with the Christian dispensation began a new era of love, justice, and mercy, assimilating closely to the primeval harmony of Eden.

Man is unquestionably right in his endeavours to prolong life, but the two methods adopted to secure this end are as wide apart as the poles are asunder. A rapid and easy death to the animal we kill for food, that we may, *with certainty*, re-invigorate exhausted nature, is very different from the infliction of long-lingering tortures, extending, perhaps, over days or weeks, with but the *remotest possibility* of prolonging life!

If the killing of animals be as justifiable in the one case as in the other, then the conclusion is that the killing by torture is no more open to reprehension than the killing by an almost instantaneous death. This is a legitimate deduction from the clerical argument—an argument that does not appear to be based upon Divine law—for if any killing is illegal in the sight of the Creator, the worst criminal is he who prolongs and intensifies the death agony.

No one will dispute that man has not, owing to his altered condition, compared with his first state, assumed a *quasi* right to deprive animals of life for food, though it is contrary to the strict letter of the Creator's law; but dare

any one assert that God has handed over the lower creation to man to ill-treat and brutally torture for the selfish purpose of endeavouring to alleviate his own self-inflicted pain? What answer can be given by any one who regards the Deity as a God of mercy, but that "None but a devil could have made them for that?"

Though we cannot fathom the mysterious workings of the Almighty Power, and though, as Milton sang—

> "To attain
> The height and depth of Thy eternal ways,
> All human thoughts come short, Supreme of Things"—

yet, the glimpses we are permitted to obtain of the Deity, as revealed in His works, combine to show us that the whole universe was originally one vast harmony, into which pain, death, or cruelty were never intended to enter.

If proof were yet wanting that any form of cruelty is repugnant to God, the Bible still furnishes us with almost endless arguments to show His mercy to His creatures. He ordained one day in every seven as a rest, not only for man but for animals; He ordered that the ox should not be muzzled "when he treadeth out the corn;" should your enemy's ass be lying under its burden, you are commanded to help and relieve it; although sparrows are sold for such small sums of money, thus making them appear contemptible, yet "not one of them is forgotten before God;" the wants of animals are attended to, for "He giveth to the beast his food, and to the young ravens which cry;" Balaam was reproved for his needless cruelty towards his ass, and, in short, "His tender mercies are over all His works." The "tender mercies" of God, as His attribute, are not consonant with the idea that He could ever sanction His creation to be made the resting-place of the incarnation of cruelty. We cannot question His mercy, but we can that of man, and the inspired Solomon leaves it on record that "the tender mercies of the wicked are cruel."

Such plain rules of conduct, embodied in the preceding quotations, towards the brute creation, admit of no doubt as to the course a moral man ought to pursue with regard to vivisection; he cannot attempt to steer a middle course; either the practice is right, or it is not right; restriction of torture within limits is but a half-hearted proceeding; if a decision be arrived at that it is not right, then nothing short of its entire abolition can satisfy the conscience.

Even if the material advantage to ourselves could be proved, the law of God still sternly condemns the cruel practice, and seems to say—"He that will save his life (by these means), the same shall lose it."

The final standpoint of the upholders of vivisection is based upon the Utilitarian doctrine of "the greatest happiness to the greatest number," which, in the subject under consideration, means—that the greatest happiness to man (*i.e.*, relief from pain) is to be obtained by causing the greatest agony to the lower animals. Jeremy Bentham and his disciples have often been taunted with the remark that they confine this happiness to the material, economic, or bodily welfare of the multitude, and studiously disregard all higher considerations. However this may be in other cases, yet in the one before us, vivisectors certainly lay the doctrine open to this imputation. Their plea, that the end justifies the means, by procuring relief to mankind through the torture of animals, is one which, viewed from the high moral elevation of the Bible, cannot be allowed to stand for a moment. The greatest happiness that vivisectors aim at is not moral but physical; they divorce morality from it, and the practice remains nothing more than a selfish transference of pain from man to animal.

But when the utility itself of these experiments is seen to have no existence, the utilitarian doctrine becomes a nonentity, and serves only to show still more plainly that vivisection and a profound moral sense are two incompatibilities which cannot stand together, but must ever be apart.

All that these cruel experiments have done has been to demonstrate how utterly barren they are of satisfactory results; therefore, let their votaries return to legitimate paths where a much more brilliant field of stable truths await them. Religion, morality, science, logic, and the results of vivisection experiments themselves, one and all exclaim against the practice, and exhibit as clearly as possible the fallacy of attempting to investigate nature by unnatural methods, involving pain to the victim and a moral degradation to the investigator.

CHAPTER VI.

HAVING now gone through a wide range of physiological subjects, with the object of showing the incongruities and fallacies to which vivisection enquiries are liable, it must appear plainly that the progress made by this method of enquiry has not been equal to the zeal and ardour with which it has been cultivated.

It is certainly to be wondered at that the human intellect should permit itself to be launched upon such a sea of perplexities as that of vivisection, when it is palpable that nothing but the most contradictory statements and opinions have been the outcome. The disturbed mind, seeking for rest in a tangible theory, cannot find it in cruel experiments, and those who adhere persistently to this method of interrogating nature, can satisfy themselves with but vague notions and unphilosophic inferences.

Putting aside, for the moment, the questions of cruelty and morality, and looking at the subject entirely from its scientific aspect, we find so many and great elements of fallacy which are, from the nature of the proceedings, bound to creep in, as to be sufficient to throw grave doubts upon the value of the results. These fallacies must exist whether the animal be narcotized or not; the narcotism induces an obvious perturbation of the system, and the knife produces a similar effect. Pain and mechanical restraint are ample to upset the animal's equilibrium, and render it unfit for the deduction of any accurate physiological law. In illustration of the truth of these assertions, the manifold disputes between vivisectors as to the correct interpretation of phenomena witnessed during experiments on the lower animals, have been adduced. The contention that vivisec-

tion is unscientific needed but little proof, for the practitioners of that form of physiology have taken especial care to well belabour each other with the imputation of having neglected to take sundry important points into consideration, and therefore of having arrived at unphilosophical conclusions! Many examples of this are to be found in the preceding pages; and we have ascertained that the labours of vivisectors have been practically in vain, for they inform us that experimentation upon most animals clouds the result by the numerous fallacies introduced: but that henceforth the monkey, grim congener of man, will form the most acceptable subject in the physiologist's laboratory, as he approaches most nearly to the human form both structurally and functionally. It is at least satisfactory to hear the authorities themselves acknowledge that hitherto vivisection has been conducted on wrong principles!

One cannot help pitying the monkey, who will in future occupy an unenviable position on the physiological altar, and the animal himself, no doubt, would curse the day when he was made in so close a likeness to upright man, who alone of all the animals is endowed with conscience and reason!

Seeing, then, that its supporters attribute so much inaccuracy and fallacy to vivisection, it becomes a foregone conclusion that experiments on living animals never have advanced physiological science; whilst, on the other hand, logical methods of research into disease and aberrations from normality, have given the clue to some of the grandest laws of our being. The patient labours of the pathologist and of the keen observer of Nature, have enriched the world with many a gem of thought and goodly benefit. Vaccination and auscultation illustrate well, by the blessings their use have bestowed upon mankind, the practical value of all other kinds of research over vivisection. Nature delights in confusing those who seek to extract knowledge from the writhing forms of poor animals; it interposes barriers by introducing so many sources of error that it becomes a mar-

vel how any one can still pursue such an extraordinary system as a means of discovery.

If we could believe in the utility of vivisection, and in its logical and scientific accuracy, we should still be met with a very serious objection to its practice in the shape of the morality of the procedure. The remarks that have been made upon this point tend to show that originally the lower animals, together with man, as parts of an harmonious whole, enjoyed freedom from pain and disease; that man is to blame for the condition of misery to which the brute creation has become reduced, and that we ought therefore to endeavour to follow the clear precepts of the Bible and treat animals with kindness, in order that we may to a certain extent realise Isaiah's vision of creation once more wrapt in its pristine harmony, and that this can never take place as long as ill-treatment makes animals distrust us.

The force of habit has been dwelt upon in order to show the danger of forming strong tendencies in a direction which is doubtful as to goodness or utility. Tendencies once formed are never eradicated; they may be kept in abeyance by stronger ones, but an appropriate stimulus may recall them once more to consciousness. This demonstrates the necessity of an education which will prevent habits of doubtful morality or cruelty. A habit may become a monomania, hence no dominant idea, to the exclusion of all others, ought to be encouraged.

Repetition of vivisection experiments serves to deaden sympathy for pain, and to engender a habit of indifference merging into cruelty itself. In illustration of this position, physiologists openly assert that medical students cannot possibly become thoroughly conversant with their profession unless they are permitted to see for themselves, in the living body, all the varieties of function in full action. This would imply such wholesale torture that the general public, somewhat apathetic at present, would rise up in indignation to stay such proceedings. The medical profession is well

aware that such a course of torture is unnecessary either to make a good surgeon or physician, and that eminence and excellence in the profession are quite compatible with being a perfect ignoramus in the experimental manipulations of physiology. Now the minds that could suggest such ideas of torture must be warped either by imagining that animals could suffer no pain, or by feeling the utmost indifference towards their sufferings. The result of all this culminates in callous cruelty, and therefore no one can be astonished that Abernethy, many years ago, should have stated publicly that these experiments tend to "harden the feelings."

Considerations such as these ought to have a deep effect on the minds of those who have hitherto doubted the propriety of the recent outcry against vivisection. This outburst is not the foolish clamour of a faction fighting for chimerical ideas, nor is it the mere outcome of a morbid sentimentality; but it is the result of a deep and growing conviction that God never has given power to man for any purpose of cruelty whatever; that nature herself bears out God's law by refusing to be so interrogated, and by mystifying the experimenters.

In the preceding pages, numerous pathological states have been adduced, so as to show that any discovery purporting to be made by vivisection could have been equally well, and, as a rule, just as rapidly, ascertained by a careful study of pathology.

It is to human and comparative anatomy that we are indebted for many great advances in physiological knowledge, as, for example, in the classification of the nerves by Sir Charles Bell; whilst to chemistry we owe a debt of gratitude for improvements in that most important branch of medicine, therapeutics, and for the insight it has given us into the nature of disease germs, and the methods of destroying their infectious properties.

To vivisection we owe nothing; it is a loathsome and horrible proceeding from beginning to end; and how

ostensibly humane men can, day after day, witness the fearful torments and agonies animals have to undergo in the physiological laboratory is a mystery known only to themselves.

Let us endeavour then, in the best and truest interests of humanity, to discard such a cruel, degrading, and barren means of investigation. The whole of creation was formed to work harmoniously; pain and cruelty are not natural to this earth; and he who tries to lessen suffering is attempting to raise the moral tone of the world, and has taken well to heart the precept that the Creator loves the *whole* of His creation. A noble career of humanity is opened out to us by vindicating the mercy of God, and by relieving suffering whether it be in man or beast. This is one of the objects of our brief sojourn in this world; time is too short to hesitate; then let us use our utmost endeavours to protect helpless animals, and to prove that vivisectors will ultimately have to adopt the saying that—"The proper study of Mankind is Man."

APPENDIX.

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ERRATA.

Page 71, line 11, "experiments," read *experimenters.*
Page 151, line 29, "as its," read *as to its.*

2071 014

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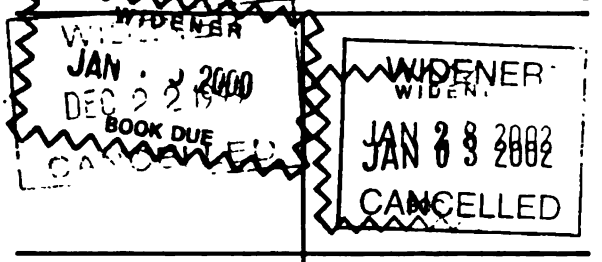

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